What happens next? Can economic forecasters foretell the future?

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WHAT HAPPENS NEXT?
CAN ECONOMIC FORECASTERS FORETELL THE FUTURE?

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Abstract

The Panel of Independent Forecasters was formed after the UK government left the European Exchange Rate Mechanism in September 1992. Its terms of reference required the Panel members to report to the Chancellor of the Exchequer on the current and future prospects for the UK economy and to make policy recommendations. This thesis is based on the meetings of this Panel during 1993. Applying theories and techniques developed in the Sociology of Scientific Knowledge (SSK), it analyses econometric modelling and forecasting at a series of levels ranging from the individual regression equation to the forecasts and recommendations made by the Panel members. The common theme running throughout is the relative roles of econometrics and expertise in the modelling and forecasting process. For example, it is shown how, in both estimation and use, econometric models are shot through with the expertise and judgement of economic forecasters. Similarly, it is shown how the skill and interpretative abilities of economists are necessary to turn 'general' economic stories, such as a 'devaluation', into a plausible forecast for the economic outcome of a particular set of circumstances. By highlighting the interpretative flexibility found in economic modelling and forecasting the thesis not only accounts for the diversity of the discipline but also forces a re-evaluation of its claims to influence. In this way, it contributes not only the development of SSK, by extending it to previously neglected social sciences, but also to policy making. In particular, by showing how econometric models and data are unable to distinguish between economic theories with different moral and political implications, the need for alternative policy structures, within which orthodoxy is not uncritically re-presented as truth, is emphasised. In short, the thesis shows how, rather than taking the politics out of economics, macro-econometric models put it in.

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Chapter 1

Economic forecasting is big business. It is supported by governments, City firms, banks and other financial institutions, as well as commercial organisations and corporations of every shape and size. Every single minute of every single day the fortunes of individuals and whole nations are gambled on forecasts for the prices of anything from coffee beans to money itself.

Because of their importance for the nation, and the impact of economic affairs on the life of every individual citizen, the way in which these forecasts are produced should be a topic of great interest. Strangely this is not the case, at least as far as sociology has been concerned. In fact, with the sole exception of Ashmore, Mulkay and Pinch's study of health economics[1], the sociology of economics has scarcely begun. This thesis begins to fill this gap by providing a sociological account of the practices and methods used by one of the most important groups of economic forecasters - those chosen to advise the government on matters of policy.

It is worth noting at the outset that the economic forecasters on which this study focuses are not intended to be representative of all economic forecasters. For example, some forecasters, known as 'chartists', study graphs of price time series for a particular product or commodity, looking for telltale patterns which can be used to predict future price movements. Others use state of the art data visualisation and statistical software to carry out sophisticated multivariate analysis of price series. Others use concepts from theoretical physics and number theory. From this diversity of forecasting methodologies, two quite distinct groupings emerge. There are those who make forecasts on the basis of a 'technical' analysis of patterns in the data series, and those who analyse the economic 'fundamentals' of the underlying situation. The first group corresponds to profit orientated traders, the second to policy orientated economists (although some traders will also use a fundamental analysis). A technical forecaster will typically analyse a particular price series in great depth but, because the analysis focuses on just a few variables, nothing can be inferred from it about the behaviour of the economy as a whole. In contrast, a fundamental analysis will use a wider range of inputs and will generally provide a narrative for the whole economy.

Another important difference between the two types of forecast is the representation of the economy which they imply. The forecasts based on physics or mathematics treat the economy as if it were an autonomous, natural system, the

fundamental analysis as if it were a social one. To give a simple example, a technical forecast for the exchange rate could be derived from an analysis based on chaos theory and fractals in which patterns identified within daily price movements can also be found in longer scale price series[2]. On the other hand, a fundamental analysis would concentrate on things like the level of activity within the two economies, their respective inflation and interest rates, as well as governments' policies. In other words, the fractal based forecast treats the exchange rate as something determined by a set of non-social rules, the fundamental analysis sees it as the outcome of a social process. For the fractal forecast the outcome is deterministic, for the economic forecast it is contingent on certain social and political events occurring. Although each approach has its merits, and (as noted) large financial institutions will typically employ both sorts of forecaster, I am primarily interested in the second group. The reasons for this are outlined below.

The Chancellor's Panel of Independent Forecasters were chosen from this diversity of approaches because they are unusual in one very important respect - they actually use economics to produce forecasts for the economy. Other forecasters, particularly the technical ones, are really only concerned with the 'bottom line' and any method which gives an advantage in the market place will be used. With this in mind, a sociological analysis which showed that the reading of charts depended on interpretation, negotiation and was hard to replicate would not be particularly interesting. For example, it could easily be dismissed as a 'soft' case study (in that it is not even clear that any 'science' is involved) and would add little new to the wider corpus of SSK[3]. The Chancellor's Wise Men represent a 'harder' case sociologically because they are explicitly using economic science and its attendant techniques to derive a theoretically informed and empirically validated understanding of the economy.

This thesis documents how the scientific process of economic forecasting is invested with the skill and judgement of the forecasters. The thesis is not critical of economic *modellers*, but it is rather sceptical of econometric *models*. The basic argument which is advanced is that it is not the economic models which produce the forecasts. Nevertheless the models are important because it is through estimating, updating and using econometric models that economic forecasters acquire the expertise necessary to make economic forecasts. It is in this sense that economic models support forecasting activity.

The thesis is structured in such a way as to mirror my own learning process and to illustrate the qualitative research methodology which underpins it. The remainder of this chapter outlines the sociological theory and research which inform the project and also provides a brief introduction to the development of macro-econometric modelling in the UK. Next, because a key part of the research methodology involved understanding economic modelling from the 'bottom up', chapters 2 and 3 are devoted to the explanation and analysis of the methods and techniques used by economic modellers. As a result economists will be familiar with much of the material covered in the early chapters, although sociologists will find it new. However, the chapters are more than a recapitulation of standard econometric texts and the material is always analysed through the lens of sociology. As such, they blend econometrics with sociological commentary and thus, I hope, economists will also find the discussion interesting.

Chapter 2 begins this process by discussing the building blocks of econometric modelling in some detail and introduces some of the more important concepts and techniques used by econometric modellers. The focus is thus on National Income Accounts, the estimation of regression equations and different types of forecast tests. One interesting feature which is highlighted in this chapter is the ambiguity of econometric testing and the need for economic modellers to have strong prior beliefs if the Experimenters' Regress is to be avoided. In other words, it is shown that econometric testing only seems convincing in those cases where the economics community has already agreed what the correct answer should be. The next chapter, Chapter 3, develops these themes by illustrating how an economic forecast is produced and examining the relative importance of econometrics and judgement in this process. The chapter shows how a forecast produced by an econometric model can be 'fine tuned' to the average of recently published forecasts through the use of residual adjustments. It is argued that the average of recent forecasts is an important reference point for economic forecasters, and further suggested that they use this consensus measure as a way of minimising the Regress caused by the fact that no-one can know what the 'correct' forecast is. The conclusion drawn is that economic forecasts are not extrapolations based on the past, but considered judgements based on expectations about the future.

Taken together, chapters 2 and 3 thus highlight expertise and skill which make economic modelling and forecasting possible. Having thus examined the uncertain

foundations of economic modelling, the next 4 chapters shift the focus to economic forecasting and examine how plausible forecasts can be produced and defended. The argument made in Chapter 4 is that, although estimating econometric equations will not produce a model of the economy which can be used for forecasting, the process is nevertheless an important one. The reason is that it is by estimating, using and updating macro-econometric models that economic forecasters acquire their expertise. It is this expertise which, in the final analysis, determines what the published forecast actually is.

Chapters 5, 6 and 7 develop these themes in more detail by examining the forecasts produced by the Treasury's Panel of Independent Forecasters at each of their three meetings in 1993. In contrast to Chapter 3, which showed how judgements can be used to reduce controversy by bringing an individual forecast closer to the average, these chapters show how controversy is created and maintained by judgements which take the forecast away from the consensus and establish more distinctive and individual forecast positions.

Finally, in Chapter 8, the conclusions are drawn. The first part of this chapter lets the forecasters reflect on their performance in 1993 and analyses the ways in which their forecasts might have been improved. This chapter thus offers some insights into the continuing success of macro-econometric modellers to secure funding despite their apparent failure to produce accurate forecasts. Following this, the thesis concludes by examining the lessons which SSK can draw from the thesis and returns to the themes of symmetry, neutrality and commitment which are raised in the next section.

Sociology of Science and Economics

Sociology of Science

The Sociology of Scientific Knowledge (SSK) is a diverse field which uses qualitative methods to examine the methods by which scientific knowledge claims are made, supported and assessed. Although there are several distinct schools and methods within the sociology of science, a principal claim of all is that scientific knowledge can be understood as a social phenomenon. Thus, there are some sociologists of science who would argue that scientific knowledge can be explained in terms of the social interests of its proponents[4]; there are others who draw on the traditions of ethnomethodology to show how knowledge claims are constructed and sustained in the everyday practices of scientists;[5] there are others who see the scientist as trying to enrol other social and natural 'actants' into a network of beliefs and practices[6]; and there are still others who

use a micro-sociological analysis of scientists' interactions and day-to-day practices to highlight the interpretative flexibility of empirical science and thus open the space within which social factors impinge upon scientific knowledge[7]. However, despite these differences, all would agree that social practices and factors of one form or another are an important constituent of scientific knowledge.

This thesis draws upon the strand of science studies which concerns itself with the empirical working through of the later philosophy of Wittgenstein[8]. In the *Philosophical Investigations* Wittgenstein rejects the logical positivism which formed the basis of his first major work, the *Tractatus*, and argues that it is the use of a concept within a form-of-life which gives it its meaning. The sociology of scientific knowledge uses this insight as the basis for analysing knowledge claims as social phenomena. The four cardinal tenets of this approach were given by David Bloor in the first book length treatment of the subject[9]. The tenets are

1. [SSK] would be causal, that is concerned with the conditions which bring about beliefs or states of knowledge.

2. It would be impartial with respect to truth or falsity, rationality or irrationality, success or failure.

3. It would be symmetrical in its style of explanation. The same types of explanation would explain both true and false beliefs.

4. It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself.

These requirements of causality, impartiality, symmetry and reflexivity are the basis of the 'Strong Programme' in the Sociology of Scientific Knowledge. The aim of the Strong Programme was (and is) to demonstrate that the perceived objectivity of scientific knowledge is actually a function of the social processes which make up and define scientific activity. The slogan of the movement was 'interpretative flexibility'[10] and its targets the 'hard' cases of prestigious sciences such as mathematics[11] and physics[12]

The aim of this original research was to demonstrate the potential of a sociological analysis in the most significant way possible. This step was important because it was generally believed that although a sociology of scientific error was possible, a sociology of scientific knowledge itself was not. Earlier sociologists of

science, such as Merton, had not thought it appropriate to analyse scientific knowledge claims as social phenomena; mistakes on the other hand, were clearly the result of social 'contaminants'. Breaking with this belief that scientific knowledge was asocial and did not admit to a sociological analysis was therefore a vital first step for the proponents of the Strong Programme. However, although they wanted to change sociology, and quite possibly society, it is not the case that sociologists of scientific knowledge necessarily wanted to change science itself. Rather, what some wanted to achieve was a situation in which the epistemic supremacy granted to science would be withdrawn as it became clear that scientists' expertise was grounded in their everyday practices and was not the result of a privileged method of discovering the truth.[13] In this new world, scientists would behave in the same way as they had in the old one, and their expertise would be respected as before. The change would be in the relationships between these experts and other social agents. Scientists' expertise would be on a par with other experts and not reified as a transcendent truth. The appearance of transcendence resulted from a misapprehension of scientific knowledge's means of production.

Once the Strong Programme had been shown to be feasible, there followed a period within which science studies expanded rapidly and the techniques developed by the first generation of SSK researchers were applied to a variety of scientific controversies. However, after this initial flurry of activity, the science studies field as a whole rather lost its way and the critical edge which characterised much of the early work became dulled. In particular, once the sociological studies of esoteric natural sciences had appeared, together with studies of applied sciences such as the forensic and medical sciences, the enterprise seems to have lost its unity. Various authors have attempted to differentiate themselves and their research through the writing of prescriptive methodological statements[14].

One issue which came to dominate these discussions concerned the extent to which Bloor's fourth tenet of reflexivity should be put into practice. The reflexivity issue, however, was just one part of a wider ranging debate concerning what should be 'taken for granted' by the sociologist and what needed to be 'explained'. The arguments thus turned on how relativist social studies of science should be and were eventually construed in terms of whether social studies of science should be philosophically or politically radical. The debate was therefore an argument about what SSK was actually for.

The proponents of reflexivity argued that because the sociology of science could itself be examined sociologically, then it should be the topic for the social studies of science. The reflexive sociologists wanted to problematise the idea of representation and hence the very accounts upon which SSK depended. This desire to relativise the production of sociological accounts led the reflexive sociologists to develop so-called 'New Literary Forms' in which different typographical and narrative conventions, together with multiple voices and characters were used to reflect the contested and constructed nature of the SSK account.

However, there were also other social scientists who wanted to be more philosophically radical than this. Unlike the reflexive sociologists, who merely wanted to increase the critical self-awareness of sociologists of science, this second group, championed by Bruno Latour and Michel Callon, wanted to dissolve the dichotomy of 'nature' and 'society' upon which SSK had originally been founded. Callon and Latour argued that although it may once have been expedient to accept and use 'natural' and 'social' as unproblematic categories, this was no longer the case. With science studies now established as a mature intellectual discipline, the way forward, they said, was to apply its techniques to the hitherto taken-for-granted categories of 'Nature' and 'Society'. Thus, just as scientific facts are socially constructed, so too is the division between natural things and social things and so these categories cannot be uncritically imported into sociological analysis. Callon and Latour developed Actant-Network Analysis to remove this Nature-Society distinction and to treat all nodes in the Network, be they social, natural or mechanical agents, as undifferentiated Actants.[15]

The philosophical radicals believed that the proponents of so-called Classical SSK were being unnecessarily conservative. The practicality and utility of a sociological analysis of scientific knowledge had been established for all to see. The time had come, so they said, to build upon this foundational work with a new research Programme which would explode the taken-for-granted categories of Classical SSK in much the same way as it had, in its day, exploded the taken-for-granted categories of mathematics and physics.

In response to these challenges, the proponents of Classical SSK argued that just because something could be done, this did not mean that it ought to be done. Thus the conservative Classicals argued that philosophical radicalism, although intellectually consistent, was not the best way for SSK, as a socio-political activity, to proceed. The

conservatives refuse to accept the premise of Callon and Latour's argument (i.e. that the point of Classical SSK has been well made) and, on the contrary, believed that there is still much to be done before sociological analyses of science are uncontroversial. In particular, they fear that philosophically radical research will be deleterious for the wider interests of SSK because granting equal explanatory status to the natural and social in their accounts actually undermines Classical SSK, which was distinguished by its advocacy of the 'social' as a determinant of scientific knowledge. Thus, philosophically radical SSK is politically conservative as it acts to reinforce the reified and canonical model of science which the Classicals thought they had dislodged[16]. Unlike the controversies studied by SSK, which invariably come to some sort of closure, this one is still very much unresolved with both polar positions enjoying support.

This thesis is located most comfortably within the Classical pole of SSK research. The reason is that although, where necessary, the distinction between model and modeller, mechanical agent and social agent is held as unproblematic, the natural world does not enter into the analysis to any great extent. Economics is a social science and deals with a social world - nature does not really enter into it at all. It is in this sense that the Nature-Society distinction is held unproblematically.

However, the focus of the thesis on a social science, as opposed to a natural one, does bring with it certain problems. Unlike the methodological angst described in the preceding paragraphs, the problems which arise with the sociological study of a social science do not relate to Bloor's fourth tenet, but to the third - that of symmetry.

Symmetry, Neutrality and the Analytic Critique of Science

The virtue of a symmetrical analysis is that it ensures that social researchers remain even-handed in their analysis of scientific controversies. The alternative is to reserve social explanations for just one side (inevitably the 'losers') and natural-empirical ones for the other (inevitably the 'winners'). The 'symmetry' principle ensures that sociologists of science do not become sociologists of error and promotes a professional agnosticism with regard to the outcome of the science being studied[17].

However, there are in fact two dimensions to the symmetry principle. The first is that the same kind of explanations are to be used to explain both success and failure, truth and falsehood. The second is that this sort of analysis is neutral, even-handed and disinterested. It turns out that this second dimension is quite distinct from the first and,

moreover, that its accomplishment poses special problems. The reason is that, despite the protestations of the sociologist, a symmetrical analysis is not always perceived as neutral by the participants in the science being studied. In other words neutrality is something which has to be accomplished and is not intrinsic to the analysis itself.

The 'symmetry' debate has thus tended to centre around the capture of the sociological account by one side in an on-going controversy and the subsequent deployment of the sociologist as a discursive resource in that controversy. The initial belief was that sociologists would always be captured by the 'underdog', and that this was more or less inevitable. The issue was what sociologists should do about this. The positions ranged from only doing research on controversies in which one's personal sympathies lay with the 'underdog' to arguments that the sociologist *qua* sociologist was committed to neutrality in their professional life, no matter what their beliefs or actions as an individual citizen. As is often the case in SSK, the more the categories 'symmetrical', 'neutral' and 'underdog' were analysed and debated, the more complicated things became.

When the dust finally settled, it seemed that the symmetry tenet did necessarily imply neutrality. Thus, in cases where the underdog was of a particularly low status, the sociologist would have to work extra hard to deconstruct the orthodoxy and make the unorthodox credible - such an endeavour would not be neutral, but it would be symmetrical[18]. Similarly, there will be other instances where sociologists of science will wish to draw upon sociological insights and research to intervene in areas where science and policy meet. In these circumstances, SSK informs an Analytic Critique of Science which is not neutral, as it is clearly intended to influence practice, but which remains symmetrical, in as much as no particular knowledge claim is supported[19].

Unfortunately, these are not quite the ways in which neutrality and symmetry are problematic in the sociological study of economic modelling. Clearly, the Analytic Critique of Science is helpful because it can legitimate the sociological critique of econometric modelling as a policy science. Thus it can move beyond a symmetrical and neutral analysis of econometric modelling, similar to that which has already been accomplished for health economics[20], and address the important questions relating to the ways in which economic models can be used in policy making. However, it is not clear that this is all that is at stake. In particular, there is an additional complication which

arises from the implicit separation within the Analytic Critique of Science of the sociologist's expertise and that of the scientists and policy makers.

The problem is that the sociologist and the economist are both social scientists, and therefore share a common object of study. However, they also have incommensurable paradigms. Thus, a neutral sociological investigation of economic modelling is problematic because the sociologist of science and the economist have different ontologies. Because of this they have very different views about what a social science is, what it can achieve and how it should be done. Basically the problem is the different conceptions of 'society' with which the sociologist of science and economist work.

For the sociologist of science, the central idea is that of a 'form of life', in which it is the language shared by the community which gives meaning to the actions of the group and its members. Different forms of life imply different norms, conventions and languages which in turn imply that different behaviours will be rational in different contexts. This relativist approach (whether it be held ontologically or merely methodologically) means that the sociologist interprets the world as a social flux, shaped and constrained by social forces.

The economist, on the other hand, invariably holds a rather different view of the world. Insofar as macro-econometric modelling is concerned, the principal difference is not the economist's preoccupation with the idea of an individual agent maximising his or her utility, as these sort of considerations tend to get lost in the aggregation. Of course, macro-economic models ought to make sense as economics, but, as we shall see, the extent to which this means being based on the rational optimising choices of individual economic agents is a moot point.

In fact, the most important difference between the economists' perspective and that of the sociologist is the former's belief that there are laws governing economic behaviour which statistical analysis can uncover. The sociologist, whilst not denying that there are undoubtedly regularities in behaviours which can be discerned, and perhaps even modelled mathematically, would be reluctant to go any further and ascribe any higher ontological status to these regularities.

The difference is thus one of the meaning attached to the statistical regularities identified by econometric analysis. To the strict neo-classical economist or econometrician, they are the result of economic agents acting according to laws of

rationality or supply and demand. Thus, in principle, the future ought to be predictable, at least in a probabilistic sense. Given a set of initial conditions and the appropriate relationships, the future of the economy can be modelled, extrapolated and forecast in much the same way as any system. To the sociologist, however, these regularities are simply patterns of behaviour which have been repeated and institutionalised in the past and might continue to be so in the future. To caricature slightly, for sociologists, behaviour is primarily normative and context sensitive, for economists it is primarily rational and maximising. Because of this basic difference in viewpoint, it is difficult for a sociologist to accept that, on its own, macro-econometric modelling is likely to reveal much in the way of economic laws.

This difference in world-view seems to bring about a position in which the sociological analysis is now neither symmetric nor neutral. Is it now just polemic? I believe the answer to this is no; the research presented in this thesis is a sociological account of economic modelling which respects its scientific content. However, because of things which I know by virtue of being a sociological analyst, this thesis is also an account of economic modelling which is critical of its methods.

SSK with Attitude

In their study of health economics, Ashmore *et al* recognise that there is a fundamental difference between sociology and economics which makes it difficult to carry out a neutral analysis. However, although they recognise the issue as important, they choose not to tackle it directly but, instead, re-affirm their commitment to neutrality:

> As symmetrical analysts in the sociology of scientific knowledge tradition (see Bloor, 1976; Collins 1985; Knorr-Cetina and Mulkay 1983; Ashmore 1989) we refuse, on principle to evaluate the epistemological status of the knowledge claims we analyse. However, as applied sociologists of expertise we find that to avoid all evaluation of health economics is as unsatisfactory as it is impossible ... it's not the epistemological status of applied economics in any abstract sense that concerns us but rather the specific moral and political implications of its underlying assumptions. This leads us to be critical of the culture of economic evaluation in almost all spheres and not least in health. But simply to get involved in writing an academic critique cum political tract, and thus damning the lot of you - no offence intended - would be to play the game you health economists play. For example we would then become involved in offering our own solutions

> for the problems of health care provision, which is something that is not our task as analysts of health economics. Furthermore, if we were to list the 'ills of health economics' in precisely the same way that you specify those of the NHS, it would be difficult for us to create any radically new approach to applied social science[21].

The problem with this approach, as I see it, is that they leave health economics exactly as they found it. This would be alright if the methods of health economists were appropriate to the subject matter of their discipline, but they are not. And what is more Ashmore *et al*, by virtue of their sociological training and expertise, ought to know this. By adhering to a particular version of SSK (i.e. symmetrical and neutral, albeit reflexive) Ashmore *et al* voluntarily deprive themselves of the capacity to make a constructive contribution to the allocation of health provision in the UK. While they may choose to defend their position on the grounds that they are only 'applied sociologists of expertise' struggling to develop a 'radically new approach to social science' this has a rather sad quality about it. To be sure they are doing SSK, but it seems as if the means of SSK have become its end.

By this I mean that a typical sociological analysis of science (any science) will usually highlight the interpretative flexibility of the empirical data the scientists are working with. Of course, the aim is not to show that scientists are cheats and charlatans, fudging and forging their results, but to show what exactly it is that makes doing science difficult. Thus, the analysis will highlight the capriciousness of experiments, their sensitivity to all sorts of seemingly unimportant conditions and the tacit knowledge and craft skills of the scientists themselves.

Experience has shown that this approach will work well, so long as the interests of the research scientists are orthogonal to those of the sociologist. Thus, whether or not high fluxes of gravity waves, cold fusion or solar neutrinos are believed to exist by the scientific community has little impact on the professional or personal lives of social scientists. Consequently the sociologist has little interest in changing the practice of these scientists. It is usually this sort of science which sociologists have in mind when they set out their pro-science credentials. Even when the topic of sociological study is an applied science, the distinction between being an 'analyst' as opposed to an 'activist' can be maintained, although it may be harder to do so. Thus, for example, science policy analysts such as Bryan Wynne or Sheila Jasanoff use their sociological analyses

of the ways various institutions deal with science to argue for alternative systems which would, they believe, do a better job.[22]

The point to note here is that in each case the subject of sociological scrutiny is a natural, not a social, science. In the case of Bryan Wynne it is environmental risk assessment and in the case of Sheila Jasanoff it is the regulation of drugs by the Federal Health Authority in the USA. However, when the topic of sociological analysis is a social science unique problems occur because now the sociologist not only has an expertise of their own with regard to the institutional arrangements within which science is conducted, the sociologist also has expertise about the substantive issues which concern the scientists in question. The problem is that the sociologist's expertise tells them that the other scientists are wrong!

If the level of analysis is concerned solely with theory choice in economics, and the ways in which the ambiguities of econometrics are discussed and resolved then an analysis which is both symmetrical and neutral in the Classic traditions of SSK can be fairly easily accomplished. In fact, much of the following analysis, in which the interpretative flexibility of a economics is foregrounded, is typical of this sort of SSK research[23]. In fact, to the extent that skill and judgement are important in the natural sciences, then the thesis can be read as supporting the scientific credentials of macro-econometric modelling. If one chooses to stop here, as Ashmore, Mulkay and Pinch did, then there is no problem. However, one is also left wondering if there was any point.

The reason of course is that, at a higher level of abstraction, sociologists of science believe that the social order is not made up through the aggregation of atomised utility maximising economic agents.[24] On the contrary, they believe that social relations are created in the nexus of ideas and action, of power and knowledge, and that these relations are not reproduced through 'laws of sociology' but through the daily rituals and norms of social interaction. Once these propositions have been accepted, the whole idea of modelling a society as if it were a collection of rational agents seems a rather peculiar thing to want to do. What is more, there are good sociological reasons for thinking that the enterprise can never deliver all that appears to be hoped of it. Surely if the sociologists know anything, they know that they ought to point this out.[25] The question is how?

One solution would be to follow the example set by Collins when writing about artificial intelligence. In *Artificial Experts*, Collins explicitly stops doing SSK and invents something called 'Knowledge Science' as a vehicle for his non-neutral though sociologically-based critique of artificial intelligence. However, and for just this reason, this approach is not a wholly appropriate model for this thesis - what sort of science would have to be invented in order to frame a sociological critique of econometric modelling? Like Ashmore, Mulkay and Pinch's self-gagging, it is an option, but not the best.

It seems that what is needed is a new form of SSK research, one in which the detailed empirical research which distinguished the first and second generations of SSK research is combined with a more distinctive evaluative stance. In other words a non-neutral form of SSK. In a review published in 1993 Trevor Pinch argues that the move towards this sort of politically and morally active sort of SSK has already begun. There is, he believes already a third generation of SSK research in which the tenet of symmetry is interpreted in new ways, so as not to imply neutrality. This third generation SSK research is exemplified by Eveleen Richards' study of Vitamin C and cancer. Reviewing her book, Pinch writes as follows:

> ...[The Vitamin C] debate, unlike many of the rather esoteric areas of science studied in second generation SSK, has consequences for us all.
>
> In such circumstances, one can understand Richards' concern to intervene in this debate. The issue of neutrality clearly becomes more germane for studies of knowledge which impact upon a much wider community than the producers of that knowledge. Whether and how many solar neutrinos exist, and whether the analyst should back one theory of the sun or another , are not likely to be of much significance to the person on the Clapham omnibus. However, medicine and cancer are something that potentially affect us all in obvious ways. If Richards is led to conclude that the randomised control trial is an inappropriate way to assess therapies which involve different regimes of care and embody different ethical standards, then why should she not be allowed to say this? It would be a tragedy if SSK was not able to contribute to some of the most importance political debates about science of our times.
>
> The difficulty with Richards' conclusion is, of course, that it seems *prima facie* to go against symmetry. It seems as if she is siding with Cameron and Pauling against the medical orthodoxy ... However, it is not clear that she is

> saying this. Rather she explicitly talks about abandoning *neutral analysis*. Ashmore and [Pinch] have argued elsewhere that symmetrical and neutral analysis are not the same things[26]. One can reject neutrality but still be symmetrical.
>
> However, there is a further issue to consider. If neutrality is abandoned does it make sense to continue with the even-handed style of narrative so typical of second and third generation SSK? In other words, is the narrative of even-handedness necessarily the best narrative for an explicit non-neutrality? The odd thing about Richards' narrative is that most of it reads like a typical second generation controversy study; the author labours diligently to show the social constitution of both sides' arguments. Finally we get the sting in the 'tale' in which Richards challenges the standard use of the clinical trial. Given Richards' goal, why not put the abandonment of neutrality up front and make the argument a straightforward one about the abolition of clinical trials? SSK would figure as a resource in this argument, but the narrative structure and the audience might be very different. However, by using the narrative style of second generation SSK studies, Richards runs the risk of irritating SSK aficionados who expect 'business as usual', and at the same time may weaken the force of her political intervention.
>
> In other words, I [Pinch] am with Richards in her abandonment of neutrality, but I wanted her to go further in making her narrative less closely wedded to second generation SSK[27].

Thus he ends the review with a call for a fourth generation of SSK research, one in which symmetrical analyses are not so bound to the conventions of second generation research and which is able to integrate the empirical and evaluative aspects of its arguments. This thesis is a response to that call. Using concepts from sociology I argue that econometric modelling is, at best, a dubious enterprise. Because of what sociologists know about ideas and actions, there are good reasons for thinking that observed regularities in economic behaviour are likely to be influenced by relatively local and short lived constellations of beliefs which a community comes to share[28]. It is this view of the social, which is not shared by the economists, which makes it difficult for a sociologist to accept that macro-econometric modelling is likely to reveal much in the way of economic laws. This thesis examines this claim in some detail and uses the

techniques and insights of SSK to not only argue that this scepticism is justified, but that it is also recognised a legitimate concern by economic forecasters themselves.

Macro Modelling in the UK

This section provides a brief outline of the development of economic forecasting in the United Kingdom, from its inception shortly after the Second World War to the present day. The focus is principally on academic macro-econometric modelling and other government sponsored research teams. The section concludes with a brief survey of some comparative research, conducted by economists, which aims to give some insights into what the research described has actually achieved.

Historical Overview[29]

Macro economic forecasting began in the United Kingdom after the Second World War when Her Majesty's Treasury (HMT) began preparing qualitative assessments of economic prospects. Initially, these forecasts were not derived from a formal econometric model but were based on the judgement and intuition of Treasury economists. With this practice established, the National Institute of Economic and Social Research (NIESR) was charged with providing an independent set of forecasts against which the Treasury's assessments could be compared. The first National Institute forecasts were produced in 1959, and like those of the Treasury, relied mainly on the judgement of the economists who prepared them. By 1961, as the original judgmental relationships were gradually replaced with estimated equations, the initially qualitative assessments of the Treasury and National Institute had become quantitative projections. It was during this period that the first quarterly model of any national economy was estimated. The economy modelled was the UK[30] and the data ran from 1948-56.

In 1965, the Social Science Research Council (SSRC) was directed to allocate public funds to macro-econometric research. The first grants were awarded to the National Institute (NIESR), the London Business School (LBS), the Cambridge Economic Policy Group (CEPG), the Cambridge Growth Project (CGP) and Southampton University. The London Business School produced its first forecasts in 1966 using a fully computerised 16 equation model. This move to computer technology was formally followed by the National Institute in 1969 and the Treasury in 1971, although both had already produced experimental computer based forecasts[31]. The Bank of England began producing its own economic forecasts in 1973. More generally,

the 1970s saw a marked growth in the number of organisations producing macroeconomic forecasts and by the end of the decade it was estimated that 99 organisations were producing economic forecasts for the UK[32].

Throughout the 1970s and 80s the SSRC (which changed its name to the ESRC during this time) reviewed its funding at regular intervals. The outcome of these reviews has been that Southampton University, the Cambridge Growth Project and the Cambridge Economic Policy Group have all lost their funding. Grants to the National Institute and London Business School have been cut, and new grants were awarded to, and subsequently withdrawn from, Liverpool University and City University Business School. In the latest (1995) funding round the ESRC, in addition to maintaining the Macromodelling Bureau at the University of Warwick, and supporting on-going research at the National Institute, the London Business School and the University of Exeter, awarded several new grants. These new grants went to researchers at the London Business School and the Universities of Cambridge, Liverpool, Oxford, Sheffield and Warwick[33].

Apart from the regular funding reviews carried out by the ESRC, other interesting changes in the institutional arrangements for UK macro-modelling have been the sustained growth in privately funded forecasting[34], and the setting up by the Government of a Panel of Independent Forecasters in November 1992. The panel, quickly dubbed the 'Seven Wise Men', met for the first time in February 1993 and its terms of reference require it to comment on economic policy and to make recommendations to the Chancellor of the Exchequer. The Panel's membership was reduced to six in December 1993, when Andrew Sentance resigned following his appointment to the London Business School, an institute already represented by David Currie. In November 1994 the Panel's membership and terms of reference were reviewed by the Chancellor, Kenneth Clarke. As a result of this review, the terms of reference were altered, so that the Panel now meet only twice a year, rather than the three meetings originally required. In addition, it was announced that the Panel members would be replaced after three years. However, in order to ensure some continuity, David Currie and Wynne Godley will leave the Panel at the end of 1995, the other four will remain until the end of 1996, when further changes will be made.

Model Development

As described in the previous section, the first economic forecasts were largely judgmental affairs. Although some econometric work was available, the forecasts themselves were prepared on the basis of an informal model of the economy, which may not necessarily even have been written down. All calculations were carried out by hand using equations which had not (usually) been formally estimated. The majority of coefficients were therefore imposed by the forecaster.

The criticisms made (at the time) of these forecasts were that the policy advice produced was invariably too late and of the wrong magnitude. The response of the forecasters was to move towards more formal modelling techniques. They did not however change the basic structure of the models which persists to this day, as can be seen from the following quote:

> It is rather striking that one can look back to the first quarterly model of the United Kingdom economy estimated by Klein, Ball, Hazlewood and Vandome, using data for the period 1948 to 1956, and find there a basic structure not unlike the models used by their successors in such places as Cambridge, the London Business School, the Treasury, the Bank of England and the National Institute. The treatment of the consumption function, the trade equations, the determination of prices, of unemployment all show a high degree of continuity and stability from one generation to the another. But this is alongside a record of relative uncertainty and instability associated with some other relationships such as those determining fixed investment, stockbuilding, interest rates and now the exchange rate.[35]

Because of this continuity it is worth outlining briefly the main features of the income-expenditure model. G. Worswick, a former National Institute research officer, describes the NIESR 'model' circa 1959 as follows[36]:

1. Real average wages were inversely related to unemployment (i.e. a Phillips Curve)
2. Employment, and hence unemployment, was determined by past movements in GDP
3. Average wages multiplied by the number of people employed gave the total wage income
4. Wage income plus other income less taxes gave total personal disposable income

5. Real disposable income was calculated by dividing the nominal value obtained for total personal disposable income by a forecast for consumer prices.

6. Consumer prices were forecast from past wages and import prices.

7. Real consumer expenditure was forecast from current and past real disposable income and the availability of credit through hire purchase controls

8. Stocks and Investment were determined by an accelerator model driven by the rate of change of output

9. Exports were determined by world trade and a measure of the UK's competitiveness.

10. Since the exchange rate was fixed, competitiveness was determined by movements in relative labour costs

11. Government spending was taken to be exogenous.

12. Total domestic demand was thus the sum of consumers' expenditure, investment (including stocks), exports and public expenditure

13. Imports were determined by the total domestic demand

14. Given imports, GDP was determined by the identity: (14) = (12) - (13)

15. Given GDP, unemployment could be calculated from step (2).

...and Change

In what follows I focus on the differences which have emerged as this basic structure has been developed by the different research groups. It will be seen that most of the changes come about through attempts to model the exchange rate. However, possibly the most significant development, the increasing use of the Rational Expectations Hypothesis, occurs not as a pragmatie response to a modelling problem, but as the result of developments within economic theory. The following discussion introduces the current generation of models and gives some idea of the 'state of the art'. The models discussed are, in alphabetical order, CEPG, LBS, Liverpool, NIESR.

Cambridge Economic Policy Group

The Cambridge Economic Policy Group differs from the other research groups in that it has always placed considerable emphasis on the properties of the model as a whole. In addition to this holistic orientation, the CEPG also criticised the excessive complexity

of the other models, arguing that there was no need for such a large number of equations.

Recent information concerning the specification of the CEPG model is sparse, partly as a result of the ESRC's decision to withdraw its grant in 1983. In the early 1980s, however the model was estimated on annual data and contained 38 behavioural equations. Theoretically the model took a strong Keynesian approach, containing no expectational variables and no way for the money supply to affect output.

More recently, Professor Godley[37] has suggested that early versions of the model have underestimated the extent to which supply side factors can constrain output[38]. It seems reasonable to suppose that some alterations have been made to the specification of the model in the light of this. The model has also been re-estimated since the UK left the Exchange Rate Mechanism in September 1992.

The London Business School

Between 1975 and 1977 the London Business School made extensive alterations to the structure and specification of its UK model. The theoretical underpinning of this revision was 'International Monetarism', according to which a rise in the UK money supply relative to that overseas causes an immediate proportional fall in the UK exchange rate (measured as the value of sterling relative to other currencies).[39]

The introduction of this international monetarist orientation necessitated the specification of new wage and price equations. These were needed in order to ensure that the falls in the exchange rate (brought about by the growth in the money supply) were matched by proportionate changes in the domestic price level. The process is:

- a rise in the money stock leads to an instant fall in the exchange rate;
- this fall in the exchange rate produces gains in competitiveness and hence output;
- the rise in the money supply simultaneously raises prices and wages;
- rises in prices and wages cancel out the gains in competitiveness and output - the economy is now back where it started but with a higher price level[40].

Thus, according to international monetarism a devaluation has no long run effect on output. This differs from the conventional income-expenditure approach, according to which the gain in competitiveness is not completely cancelled out by the rise in domestic prices and wages. This monetarist specification was originally implemented

by taking the money supply as an exogenous policy variable, but later versions of the model endogenised this by letting money supply be determined by government borrowing (PSBR)[41].

In addition to this theoretical realignment other changes were also made during the 1970s. The consumption function was augmented by including inflation as an explanatory variable. The inverse relationship between consumption and inflation was supposed to model the way in which, as inflation rises, people save more in order to preserve the value of their financial assets. This change in specification is now common to almost all UK models and has the effect that, depending on the influence of inflation, the expansionary effects of higher government spending can be cancelled out by increased consumer saving as a result of the inflation created. In this way, the model can explain how the fiscal expansion of the 70s produced higher inflation but no gains in output.

During the 1980s the LBS model was revised once more. The international monetarist flavour was retained, and the revisions mainly took the form of the addition of a detailed financial sector. The aim of this was to improve the modelling of the exchange rate. In the new LBS model the exchange rate was modelled as a market clearing price determined in the financial sector under the assumption of rational expectations. Other alterations made included an improved treatment of the labour market and the explicit inclusion of wealth effects in the consumption function. In the third ESRC review, Wallis *et al* summarise the 1985 LBS model as:

> [an] aggregate quarterly model covering 770 variables (of which 70 are exogenous), with a little over 100 behavioural equations[42]. The model has a separate financial sector containing 1/3 of the total number of variables. It is based around the income-expenditure framework, but is often referred to as an 'international monetarist model ... Forward consistent expectations are assumed in the financial markets.[43]

More recently the LBS model has once again been revised substantially. In particular the assumption of rational expectations has now been dropped and replaced by a model of learning based on a 'reduced, reduced form'[44] model of the economy which economic agents are assumed to know, use and modify[45].

Liverpool University Research Group in Macroeconomics

The Liverpool model was first used for forecasting in 1980, and incorporated several (if not all) the recent developments in economic theory. The model was a 'new classical' computable general equilibrium model, solved using a rational expectations algorithm which forces expectational variables to take values which are consistent with the computed outturn values. Unlike the LBS and NI the Liverpool model includes rational expectations in all markets. The model is monetarist in the sense that higher monetary growth directly increases inflation. Structurally however the model was not very different to the mainstream models - the main differences were that it was estimated on annual rather than quarterly data and that the dynamic specifications were relatively simple.

What particularly distinguished the Liverpool model from its peers was that factors affecting the supply of labour, in particular the level of unemployment benefit, were treated in much more detail than in the demand oriented mainstream models[46]. Also, an explicit allowance for the impact of changes in wealth on consumption was made, and private expenditure decisions were not related to income as in the other models[47]. Government spending is determined endogenously on the basis of a constant PSBR/GDP ratio and an assumption about the average tax rate. This contrasts with the majority of other models which typically take government spending as an exogenous policy variable.

The exchange rate is modelled using the theory of 'uncovered interest parity' according to which any expected appreciation in the exchange rate must be matched by UK interest rates being lower than foreign ones by exactly the size of the expected rise. Finally, exports and imports are modelled jointly as a function of world trade, domestic income and the actual and expected exchange rates. This contrasts with the majority of other models which have separate import and export functions.

The Liverpool model has remained relatively unchanged during the last decade or so. However, the model is now run using quarterly and not annual data.

The National Institute for Economic and Social Research

The National Institute model, like the LBS model was also modified throughout the 1970s and 80s, although unlike the LBS it did not undergo any major theoretical re-alignment. It thus remains in the Keynesian income-expenditure tradition, and can be viewed as a quantity adjustment model, driven more by expenditures than by prices[48].

In 1977 the National Institute introduced a simple financial system to the model and, in conjunction with this, modelled the exchange rate according to the theory of long run purchasing power parity. In the NIESR the exchange rate was determined by UK prices relative to overseas prices, the visible trade balance and the covered interest rate differential between UK and US interest rates[49].

Like the LBS, the National Institute also introduced the rational expectations hypothesis into its model during the 1980s. By 1985, NIESR had forward looking behaviour in the equations for employment, stockbuilding, wages, exchange rate, and the demand for narrow money (M1). However, the exchange rate equation remained problematic for some considerable time, with the forecast rate being highly dependent on the terminal conditions set for the expected exchange rate. In practice the exchange rate is therefore exogenous in the NIESR model (of 1985).

More recent revisions[50] of the model (now Version 11.4) are:

- key price and wage equations are both forward looking, and are based on explicit theories of dynamic adjustment;
- the real interest rate, working through the cost of stockbuilding, plays an important role in influencing prices. In the longer run, real interest rates influence the model's NAIRU (Non-Accelerating Inflation Rate of Unemployment) in the same way;
- new disaggregation of output by sector and vintage production technology expanded to cover distribution and business services;
- unemployment is now modelled according to claimant status, rather than part of a labour supply decision. Unemployment is no longer therefore the most useful measure of excess supply in the labour market when it comes to explaining earnings;
- the addition of a detailed set of equations for the capital account, with relationships for direct and portfolio investment inflows and outflows and net overseas liabilities for domestic banks.

Summary

The preceding discussion has highlighted the similarities and differences between some of the macro modelling groups in the UK. It has described how, starting from the

income-expenditure model of the 1950s macro models in the UK have developed. However, despite their common heritage, the contemporary macro-econometric models do differ from each other in several important respects.

The most significant differences are ones of theoretical orientation: CEPG adopted a strong Keynesian perspective; NIESR a more eclectic Keynesianism; the LBS a short run Keynesianism, tempered by a long run international monetarism; Liverpool a new classical, monetarism[51]. The treatment of expectations, and in particular the use of the rational expectations hypothesis can also be used to discriminate between the models. CEPG has no role for expectations at all; the LBS used rational expectations for a while but subsequently abandoned them; NIESR although slower than the LBS to adopt rational expectations now uses them quite extensively[52]; Liverpool has always had rational expectations throughout. Thus, although the income-expenditure framework is basic to all the models, there is no common specification. For example, some models are specified in such as way that increases in UK money supply lead directly to increases in wages and prices. In others however no such linkage occurs.

The problem is that all the models pass the appropriate sets of tests for statistical significance, and are all equally 'good' explanations of economic events. The first question my research seeks to answer is thus what persuades economists which, of this set of statistically satisfactory models, can be rejected. One solution to the possible conundrum might be to ask if models can be ranked according to their ability to generate accurate forecasts of economic events. In the rest of this introduction I consider the extent to which forecasting accuracy can guide us in choosing a macro-econometric model. To pre-empt the conclusion slightly I will argue that, on the basis of *ex ante* forecasts, no model is unambiguously superior to the rest.

Comparative Research

The following discussion focuses on the accuracy of forecasts produced during the 1980s. It is intended to be illustrative, and does not claim to represent a full survey of the literature. Initially short-term forecasts made in 1979 and 1980 are considered, after this forecasts made in 1983 are examined. The analysis concludes by considering long term projections made in 1986, and considers the success with which forecasters were able to predict the boom of the late 1980s and the subsequent recession.

Short-term Forecasting: 1979-80

The following is based on Holden's[53] summary of a comparative study originally published by Barker[54]. Barker compared the forecasts produced by Cambridge Econometrics, Cambridge Economic Policy Group, Liverpool University Research Group, London Business School and the National Institute in 1979 (1980 in the case of Liverpool) for the following 3 years. The forecasts for the growth of GDP are shown in Table 1 below:

Table 1: Forecasts for GDP growth (% change p.a.)[55]

forecaster	forecast date	1979	1980	1981	1982
CE	March 1979	0.8	1.8	2.3	1.9
	June 1979	-0.7	-0.5	-0.3	0.2
LBS	Feb. 1979	2.4	3.0	2.0	0.2
	July 1979	1.6	-0.4	1.7	3.4
NIESR	May 1979	1.6	2.1	-	-
	Aug. 1979	0.5	0.5	-	-
CEPG	April 1979	3.0	1.6	0.2	0.9
	April 1980	1.1	-6.1	-3.6	-1.3
LPOOL	March 1980	-	0.0	1.6	2.9
Outturn		**2.5**	**-2.8**	**-2.3**	**1.0**

The study is interesting because it compares forecasts made before and after the election of the Conservative Government in 1979. By comparing forecasts made before the now famous budget of June 1979[56] with those made afterwards it is possible to see how well the forecasters managed to predict the effects of the new policies.

A preliminary inspection suggests that the forecasters correctly anticipated the contractionary effects of the government's policies, with all the groups revising the forecasts down after the June budget. However it is clear that there was very little consensus on the severity of this effect. In addition, none of the groups appear particularly close to the actual outcomes. Liverpool and the National Institute both failed to predict the recession at all, with NIESR even suggesting that output would rise by 0.5% in 1980 when in fact it contracted by 2.5%

Of the others, CE forecast the onset of recession a year early and underestimated its depth, and suggested the economic situation would improve between 1979 and 1980 when the rate of growth actually fell by over 5%. The LBS correctly forecast the onset of recession in 1980, but underestimated its depth and duration. CEPG correctly forecast the onset and duration of the recession but overestimated its severity,

forecasting a contraction of 6.1%, against an outcome of -2.8%. The conclusions drawn by Barker were:

1. the best forecasts for GDP were produced by Cambridge Econometrics, for unemployment by CEPG and for inflation by Liverpool;
2. none of the forecasters were able to predict the recession of 1980-81 with any accuracy.

Short-term Forecasting: 1983-4

In 1987 the ESRC Macromodelling Bureau at Warwick published its third review of the UK macroeconomic models[57]. Chapter 4 of this edition contains a detailed *ex post* analysis of 1 and 2 year ahead forecasts made in the autumn of 1983. The forecasts and the outturn data for GDP growth, inflation and unemployment are shown in Table 2. These forecasts are particularly interesting as March 1984 saw the beginning of the miners strike, which lasted approximately 12 months. The miners' strike, in contrast to the reform Programme of the Thatcher government is an example of an unexpected shock to the economy of which the forecasters had no knowledge and which could not be allowed for when the forecasts were prepared. Official figures suggest that the direct effect of the miners strike was to reduce output growth by 1% in 1984 and to increase it by the same amount in 1985.

Inspection of Table 2 (overleaf) however reveals that although the miners strike was a common source of error for all forecasters, it does not seem to help explain the forecasts. All the forecasters, with the single exception of Liverpool's forecast for the 1984 growth rate of GDP (expenditure measure), *underestimate* the actual growth rate. The National Institute, once again, seems to be the furthest away from the actual outcome, predicting a sustained slow down in GDP (output measure) growth, during a period in which the growth rate grew from 3 to 3.4%. The performance of the LBS is more difficult to assess, as it depends on whether one uses the output or expenditure measure of GDP. The expenditure forecast is the numerically more accurate, but the output measure suggests a steady, if not spectacular, growth in output. In contrast to the LBS and NIESR, the Liverpool group are rather optimistic, predicting growth over 3% for the entire period[58].

With regard to inflation, the LBS and NIESR seem over pessimistic, with Liverpool, in contrast, relatively optimistic. In the case of unemployment, NIESR's prediction of a sustained slow down in GDP led to an over prediction of about 200 000

by the end of 1985. LBS and Liverpool on the other under predicted the final total by 400 000 and 800 000 respectively. As unemployment actually rose throughout the entire period, the NI's pessimism on GDP, which led it to forecast that unemployment would rise steadily throughout the period in question now seems more warranted[59].

Table 2: Autumn 1983 forecasts and outturns[60]

		1983	1984	1985
GDP Growth[1]	LBS	1.8	2.4	2.4
	NIESR	2.2	2.0	1.0
Outturn		**3.0**	**3.1**	**3.4**
GDP Growth[2]	LBS	2.5	1.9	2.4
	LPOOL	3.6	3.5	3.0
Outturn		**3.7**	**1.7**	**3.3**
Inflation[3]	LBS	5.6	5.9	6.3
	NIESR	5.8	5.8	6.1
	LPOOL	4.6	3.3	2.1
Outturn		**5.2**	**4.6**	**5.4**
Unemployment[4]	LBS	2.9	3.0	2.8
	NIESR	2.9	3.1	3.4
	LPOOL	2.9	2.7	2.4
Outturn		**2.9**	**3.0**	**3.2**

[1] output measure, % change p.a. [2] expenditure measure, % change p.a.

[3] % p.a. [4] millions

The more general conclusions drawn by Wallis *et al* are that Liverpool and LBS produce the most accurate forecasts for the *level* of GDP, with LBS producing the more accurate forecasts for the *growth* of GDP. LBS and NIESR produce the best forecasts for inflation, but tend to over predict. Liverpool on the other hand tends to under predict, and by a bigger margin. Although NIESR and LBS forecast the number of unemployed more accurately they make large mistakes in forecasting the number of people actually in work. Liverpool, although wayward in their estimates of unemployment make broadly 'equal and opposite' mistakes forecasting employment.

In a paper published in 1991[61] Wallis and Whitley repeat this analysis for forecasts published during the period 1984-88[62]. The conclusions they reach are rather similar to those for 1983 forecasts. Wallis and Whitley find that all the groups have a tendency to underestimate the rate of growth of output during this period, with only 4 out of 46 forecasts appearing over-optimistic.

However, some pairwise differences do emerge in the errors made by the different groups. The demand based models of the LBS and NIESR tend to be more pessimistic than the supply-side models of Liverpool and the City University Business School. LBS and NIESR have average errors of about 3% for the level of output and 1.5% for the growth rate. Liverpool and City University in contrast have errors of approximately 1% and 0% for the level and growth of output respectively.

For inflation, Liverpool has a tendency to under predict while the others over predict. Errors on inflation are less systematic than those for output however, with the LBS forecast errors changing sign. Overall, the worst forecasts for inflation are by NIESR.

Considering all four of the main variables as equally important, the best 1 year ahead forecasts (defined as having the lowest average root mean squared error) are produced by the LBS, with Liverpool producing the best 2 year ahead forecasts. Taking the variables individually, Liverpool and City University produce the best forecasts for the level and growth of output, LBS for inflation and NIESR (just) for unemployment. The average root mean square errors for all the models are shown in Table 3.[63]

Table 3: Average RMSE for 1984-8 (all models)[64]

	1 year	2 year	
GDP (level)	1.9%	3.1%	of actual
GDP (growth)	1.2%	1.5%	per annum
Inflation	0.9%	1.7%	per annum
Unemployment	0.8%	1.9%	of actual

The size of the RMSE's shown in table 3 are all large relative to the variable in question. For example, the average growth rate for GDP over that last 20 years is about 2.5%, although for the period in question it was admittedly about 4%. Nevertheless, a 1.2% error in forecasting a growth rate of even 4% is still a considerable margin. Trend GDP growth of 2.8% is quite different from boom economy of the late 80s. Similarly for the other variables, the errors involved are all of the same magnitude as the outcome being predicted.

Longer term projections

The studies discussed above considered only short term forecasts, i.e. forecasts for 1 and 2 years ahead. In this section I will consider the longer term projections also produced by most forecasting groups. Table 4 (below) shows the 5 year projections made in late 1986 by the LBS, NIESR and Liverpool groups, as well as the outturn figures.

Table 4: 5-Year Ahead Forecasts[65]

		1986	1987	1988	1989	1990	1991
GDP Growth	LBS	2.1	3.0	3.2	2.8	2.2	2.1
	LPOOL	2.6	3.5	2.2	2.9	2.3	2.8
	NIESR	2.5	2.1	1.5	1.6	1.1	1.0
Outturn		**3.3**	**4.7**	**4.3**	**2.3**	**1.1**	**-2.5**
Inflation	LBS	3.6	3.3	4.4	4.6	3.9	2.8
	LPOOL	3.8	3.1	2.3	2.1	1.7	1.2
	NIESR	3.7	4.8	6.3	6.8	6.0	5.2
Outturn		**3.4**	**4.2**	**4.9**	**7.8**	**9.4**	**5.9**
Unemployment	LBS	3.2	3.2	3.2	3.1	3.0	2.9
	LPOOL	3.2	3.0	2.9	2.9	2.8	2.5
	NIESR	3.2	3.0	3.0	3.0	3.1	3.1
Outturn		**3.1**	**2.8**	**2.3**	**1.7**	**1.6**	**2.2**

A cursory inspection of Table 4 shows how the rapid growth in output which occurred in 1987 and 1988 took most forecasters completely by surprise. The economic downturn which starts in 1989 and turns into a full blown recession in 1991 is similarly unanticipated. The doubling of the inflation rate which took place between 1987/88 and 1990 is also not forecast by any of the groups

The disparity between the outturn data and the forecasts has led one economist[66] to conclude that the forecasts have been of 'no value'. However, it should be noted that a detailed study of forecast performance by the National Institute[67] concludes that the NIESR forecasts for the period 1968 to 90 are generally both 'efficient' and 'unbiased', suggesting that they do contain information significant for prediction. However:

> a less encouraging finding was that many of the forecasts made during the 1980s *did* exhibit some degree of bias and inefficiency, and it seems that forecasts for some variables contained little useful information. We therefore applied a test for stability to the relationship between forecast and outturn before and after 1983. Rather surprisingly, in most cases the hypothesis of stability could *not* be rejected. It is possible to argue for most variables that forecasting performance is, in some underlying sense, just as good as it has ever been - we have just been unlucky in the last few years. (p. 2, emphasis in original.)

Conclusions

So far I have set out a brief history of the development of macro models in the UK and evaluated their relative performances. Firstly, a general overview was presented and some of the theoretical similarities and differences between the research groups were outlined. Next, it was shown that, using out of sample forecasting performance as a criterion, no one model or modelling team appears to have a significantly greater degree of success than the rest. Despite this forecasting and modelling activity and research continue to grow apace. The current ESRC funding round has allocated £3.2 million to macro-modelling research[68] and the Treasury's monthly survey of economic forecasts lists over 30 contributors, and this list is by no means exhaustive.

This somewhat paradoxical situation is the starting point for my research. The questions I want to answer are:

1. what reasons do macro-modellers give for their model specifications in the absence of any conclusive forecasting experience;
2. what role does falsification play (if any) in macro modelling;
3. how are the errors and radical uncertainty inherent in forecasting managed and minimised;
4. how are economic forecasts produced.

Notes

[1] Ashmore, M., Mulkay, M. and Pinch, T. (1989) *Health and Efficiency: A Sociology of Health Economics.* Milton Keynes: Open University Press.

[2] Davidson, C. (1994) 'Heads for figures: Theoreticians are in great demand in the City' *The Guardian*, 9 June, 1994

[3] In might however provide an interesting update to the rationality debates and a refreshing change from the Azade 'Poison Oracle'.

[4] see e.g. Shapin, S. and Schaffer, S. (1985) *Leviathan and the Air-Pump : Hobbes, Boyle and the Experimental Life.* Princeton, N.J. : Princeton University Press.

[5] see e.g. Garfinkel, H., Lynch, M. and Livingstone, E. (1981) 'The Work of Discovering Science Constructed with Material from the Optically Discovered Pulsar' *Philosophy of the Social Science*, **11**, pp. 131-58; Lynch, M., Livingstone, E. and Garfinkel, H. (1983) 'Temporal Order in Laboratory Work' in Knorr-Cetina, K. and Mulkay, M. (Eds.) *Science Observed: Perspectives on the Social Study of Science.* London Sage.

[6] see e.g. Callon, M. (1986a) 'The Sociology of an Actor-Network: The Case of the Electric Vehicle' in Callon, M., Law, J. and Rip, A. (Eds.) *Mapping the Dynamics of Science and Technology* (pp. 19-34). Basingstoke, UK: Macmillan; Callon, M. (1986b) 'Some Elements in the Sociology of Translation: Domestication of the Scallops and the Fishermen of St. Brieux Bay' in Law, J. (Ed) *Power, Action Belief: A New Sociology of Knowledge?* (Sociological Review Monograph, pp. 196-229). London: Routledge and Kegan Paul.

[7] see e.g. Collins, H.M. (1985) *Changing Order: Replication and Induction in Scientific Practice.* London: Sage.

[8] For a sociologically informed reading of Wittgenstein see e.g. Bloor, D. (1983) *Wittgenstein: A Social Theory of Knowledge.* London: Macmillan

[9] Bloor, D. (1976) *Knowledge and Social Imagery.* London: Routledge and Kegan Paul. pp. 4-5.

[10] Collins, H.M., op cit. note 7

[11] Bloor, D. (1973) 'Wittgenstein and Mannheim of the Sociology of Mathematics'. *Studies in the History and Philosophy of Science*, **4**, pp. 173-91; MacKenzie, D.A. (1981) *Statistics in Britain: 1865-1930.* Edinburgh: Edinburgh University Press.

[12] Collins, H.M. (1974) 'The TEA Set: Tacit Knowledge and Scientific Networks'. *Science Studies*, **4**, pp. 163-86; Collins H.M. (1975) 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics'. *Sociology*, **9**, pp. 205-24. Both articles are reprinted in the reader: Barnes, B. and Edge, D. (1982) *Science in Context: Readings in the Sociology of Science.* Milton Keynes: Open University Press.

[13] In other words, the target was not science itself, but the positivist images and philosophies through which science was mis-represented.

[14] The first attempt to systematically collect the divergent programmatic stains of science studies into a single volume came in 1983, the second in 1992. See: Knorr-Cetina, K. and Mulkay, M. (eds.) (1983) *Science Observed: Perspectives on the Social Study of Science.* London: Sage; Pickering, A. (ed.) (1992) *Science as Practice and Culture.* Chicago and London: University of Chicago Press.

[15] see e.g. Callon, M. and Latour, B. (1992) 'Don't Throw the Baby Out With the Bath School! A Reply to Collins and Yearley' in Pickering, A. (Ed.) *Science as Practice and Culture* (pp. 343-368). Chicage: University of Chicago Press.

[16] For more details on this debate see: Pickering, op cit. note 14

[17] This section owes much to: Ashmore, M. (1993) 'Ending Up on the Wrong Side: Must the Two Forms of Radicalism Always be at War'. Unpublished manuscript, prepared for the Society for the Social Studies of Science annual meeting, November 1993, Purdue University, Indiana.

[18] op cit. note 17.

[19] Collins, H.M. and Pinch, T.J. 'Unwanted Children: Essays in the Sociology of Fringe Science'. Unpublished manuscript.

[20] op cit. note 1

[21] op cit. note 1 p. 187.

[22] see e.g. Jasanoff, S. (1992a) 'Science, Politics and the Renegotiation of Expertise and EPA' *Osiris*, **7**, pp. 1-23; Jasanoff, S. (1992b) 'What Judges Should Know About the Sociology of Science' *Jurimetrics*, **32**, pp. 343-359; Wynne, B. (1989) 'Sheep Farming After Chernobyl: A Case Study in Communicating Scientific Information'

Environment Magazine, **31** and **32**, pp. 10-15, 33-39; Wynne, B. (1992) 'Uncertainty and Environmental Learning: Reconceiving Science and Policy in the Preventative Paradigm' *Global Environmental Change*, **2**, pp. 137-154.

[23] See: Scott, P., Richards, E. and Martin, B. (1990) 'Captives of Controversy: The Myth of the Neutral Social Researcher in Contemporary Scientific Controversies', *Science Technology and Human Values*, **15**, pp. 474-94; Collins, H.M. (1991) 'Captives and Victims: Response to Scott, Richards and Martin', *Science Technology and Human Values*, **16**, pp. 249-51; Scott, P., Richards, E. and Martin, B. (1991) 'Whose a Captive? Whose a Victim? Response to Collins's Method Talk', *Science Technology and Human Values*, **16**, pp. 252-5.

[24] To be fair, this point has also be made by some economists. See e.g. Paul Ormerod (1992) 'Crisis in the Exchange Rate' *Times Higher Education Supplement*, 18 September 1992, pp. 18 & 17.

[25] Again, some economists also make similar points. See Chapter 4.

[26] Ashmore, M. (1993) 'The Theatre of the Blind: Starring a Promethean Prankster, a Phoney Phenomenon, a Prism, a Pocket, and a Piece of Wood', *Social Studies of Science*, **23**, No. 1, pp. 67-106; Pinch, T and Ashmore M. (1992) 'Defending the Daft and the Dangerous: Wilheim Reich and the strange case of Orgone Energy', paper presented to the Society for Social Studies of Science annual meeting, Gothenburg, Sweden, 13 August 1992.

[27] Pinch, T. (1993) 'Generations of SSK. Review of Richards, *Vitamin C and Cancer* and Sapp, *Where the Truth Lies*'. *Social Studies of Science*, **23**, No. 2, pp. 363-73.

[28] As Donald MacKenzie has pointed out, this is not to say that processes such as 'globalisation' are unimportant

[29] This discussion draws heavily on chapters 1 and 7 of Keating (1985). I am greatly indebted to Andrew Britton for alerting me to this source: Keating, G. (1985) *The Production and Use of Economic Forecasts.* Methuen: London and New York.

[30] Klein, L.R., Ball, R.J., Hazlewood, A. and Vandome, P. (1961) *An Econometric Model of the UK* Oxford: Basil Blackwell

[31] The increasing use, and power of, computer technologies had some important effects on macro-economic models. Throughout the early 1970s the models expanded rapidly, with much of the research effort being devoted to the estimation of individual equations. A notable exception to this trend, however, was the Cambridge Economic Policy Group who focused their efforts on the properties of the model as a whole.

[32] Cyriax, G. (1981) *World Index of Economic Forecasts.* Farnborough, UK: Gower.

[33] These changes are discussed more fully in Evans R.J. (1993) *Soothsaying or Science: Falsification, Uncertainty and Social Change in Macro-econometric modelling.* Unpublished dissertation for M.Sc. in Social Research, University of Bath.

[34] The current monthly survey published by the Treasury includes forecasts from over 30 forecasting groups and this is by no means exhaustive.

[35] Britton, A.J. (ed.) (1983) *The National Institute Model of the UK Economy.* London: Heinmann.

[36] The details of the informal income expenditure framework on which these early models are based has been reconstructed by Keating (op cit. note 1, p. 136-7) from the introduction to Surrey (1971): Surrey, M.J.C. (ed.) (1971) *The Analysis and Forecasting of the British Economy.* Methuen: London and New York.

[37] Prof. Godley's research is now supported by the Leverhulme Trust

[38] Interview, 5 April 1993.

[39] The theory of international monetarism assumes that a higher UK money supply will lead to higher UK prices and wages. In addition, a secondary assumption of the theory is that 'long run purchasing power parity' holds. Because of this secondary assumption, which means that the real exchange rate will eventually return to some long run level, a rise in the UK money supply causes the foreign exchange markets to mark sterling down straight away, rather than take an anticipated capital loss some time in the future.

[40] According to the LBS equations most of this effect occurs within 2 or 3 years.

[41] It is interesting to note that by changing essentially 3 equations, those determining the exchange rate, prices and wages, the whole character of the model was changed from a conventional income-expenditure model to a international monetarist model. A corollary of this is that by over-riding a few equations the international monetarism is lost.

[42] Writing in 1989 however, Ken Holden describes the LBS model as containing over 160 behavioural equations, reflecting Keynesian properties over the short run and monetarist ones over the long run. The number of variables however seems to stay exactly the same: Holden, K. (1989) 'Macro-economic Models', in Greenaway, D. (ed.) *Current Issues in Macro-economics.* Macmillan Education: Basingstoke and London.

[43] Wallis, K.F. (ed.), Andrews, M.J., Fisher, P.G., Longbottom, J.A. and Whitely, J.D. (1987) *Models of the UK Economy: Third review by the ESRC Macromodelling Bureau.* Oxford University Press: Oxford. pp. 5-7

[44] A 'reduced form' is a version of the macroeconometric model in which only exogenous variables appear on the right hand side of equations.

[45] See the recent review by the ESRC Macroeconomic Modelling Bureau: Church, K.B., Mitchell, P.R., Smith, P.N. and Wallis, K.F. (1993) 'Comparative Properties of Models of the UK Economy'. *National Institute Economic Review*, August 1993, pp. 87-100, esp. Box A, p. 89.

[46] Although many people agree that the supply side effects identified by the Liverpool group exist, their magnitude is frequently disputed. The differences between economists on these issues are more usefully considered to be ones of degree rather than principle.

[47] Most modern macro-econometric models allow for some wealth effect on consumption. Again the differences are of degree rather than principle.

[48] Forecasts for monetary aggregates are produced but appear only as dead ends in the flow chart: they do not (at least in version 6 of the model) feed back to influence other variables.

[49] The covered interest rate differential term was intended to proxy expectations of the future exchange rate. The abolition of exchange controls in 1979 however means that the differential now reflects only arbitrage costs, which are small. Consequently NIESR has had to find other ways of modelling exchange rate expectations.

Although both LBS and NIESR models exhibited long run purchasing power parity they did so in different ways. In the LBS model, long run PPP was imposed indirectly through the price equation, in the NIESR model, long run PPP is a property of the exchange rate equation itself.

[50] The details are taken from the *National Institute Review*, November 1989.

[51] Details of model specification, and references for further reading, can be found in the regular reviews published by the ESRC Macromodelling Bureau. The first of these is: Wallis, K.F. (ed.), Andrews, M.J., Fisher, P.G., and Whitley, J.D. (1984) *Models of the UK Economy: A Review by the ESRC Macromodelling Bureau.* Oxford: Oxford University Press.

[52] According to the March 1994 ESRC Macromodelling Bureau Newsletter, the National Institute is now the only ESRC funded research group which uses model-consistent or rational expectations.

[53] Holden, op cit. note 42

[54] Barker, T. (1985) 'Forecasting the Economic Recession in the UK: A comparison based on *ex ante* forecasts'. *Journal of Forecasting*, Vol. 4, pp. 133-51

[55] Source: Holden, *op cit.* note 42, p. 174

[56] In their budget of June of 1979, the new administration introduced a series of reforms which included:

- increasing VAT from 8 to 15%;
- reducing the standard rate of income tax from 33 pence in the pound to 30;
- rejecting short-term demand policies;
- enforcing tighter controls on public spending;
- the exclusive use of monetary policy to control inflation;
- allowing the pound to float freely in the foreign exchange markets.

[57] Wallis *et al* op cit. note 43.

[58] The fact that Liverpool produced the most accurate forecast for the base year was undoubtedly aided by the fact that their forecast was published 3 months after the others.

[59] It is of course extremely difficult to judge forecasts without reference to some sort of 'big picture'. The National Institute may have got the rise unemployment (almost) right but they only did so because they were forecasting below trend growth. Given that the period saw above trend growth of over 3%, it is possible to argue that they were basically wrong, but managed to get unemployment right by chance. The same of course applies to all the other forecasting groups.

[60] Source: Wallis *et al*, op cit. note 43.

[61] Wallis, K.F. and Whitely, J. (1991) 'Sources of Error in Forecasts and Expectations: UK Economic Models, 1984-8', *Journal of Forecasting*, Vol. 10, pp. 231-253.

[62] The period 1984-88 was characterised by vigorous economic expansion, with GDP growing by about 4% per annum, far in excess of any comparable period in the previous 20 years. Unemployment rose from just under 10.5% in 1984 to 11.25% in 1986 before falling back to just under 8% in 1988. Inflation was relatively low throughout the period, declining until 1986 and picking up somewhat thereafter. This variation was not great however, with the annual estimate remaining within the range 3.5% to 5.1%.

[63] The way in which these results are resented are quite revealing. By specifying the RMSE for GDP growth in terms of the annual growth rate and not of the actual growth rate give the impression that the errors are all fairly small, i.e. less than about 3%. However, GDP growth is, on average, about 2.5 %, and error of 1.2% per annum corresponds to an error of about 50% of actual. Textual devices such as these are one the ways in which economists mange the extreme uncertainty which surrounds their forecasts.

[64] Source: Wallis and Whitely, (1991), op cit. note 61.

[65] sources: forecasts from Holden, 1989, op cit. note 53, p. 179; outturn data from *NIESR Review*

[66] Ormerod, P. (1992) 'On the Dynamics of Capitalism'. Paper presented to the Macro and Money Study Group Conference, Imperial College, London, September 1992. Quote taken from page 1 of the provisional draft.

[67] Britton, A.J. and Pain, N. (1992) *Economic Forecasting in Britain.* National Institute Report Series, Number 4. See also: Pain, N. and Britton, A.J. (1992) 'National Institute Economic Forecasts 1968 to 1991: Some tests of forecast properties', *National Institute Economic Review.* Vol. 141, August 1992, pp. 81-93.

[68] Source: ESRC 'Social Science' Newsletter, April 1995, p. 2.

Chapter 2

'Thus if the animal spirits [of investors] are dimmed and the[ir] spontaneous optimism falters, leaving us to depend on nothing but a mathematical expectation, enterprise will fade and die; though fears of loss may have a basis no more reasonable than hopes of profit had before.... We should not conclude from this that everything depends on waves of irrational psychology.... We are merely reminding ourselves that human decisions affecting the future, whether personal or political or economic, cannot depend on strict mathematical expectation, since the basis for making such a calculation does not exist; and that it is our innate urge to activity which makes the wheels go round' (Keynes, The General Theory of Employment, Interest and Money pp. 161-3)[1]

Introduction

Economic forecasting is about predicting the future and the question this chapter addresses is why it is believed to be possible, at least in some circumstances, to make predictions about what will happen next week, next month or even next year. What kind of futurology are economic forecasters engaged in? Although some futurologists are generally believed to be more plausible than others, a common link between all is the plausibility of the idea that the system, be it physical or social, is in some sense deterministic, or at least probabilistic. The source of this driving, guiding force may be internal to the system, like the laws of nature, or external, like the omnipotent deity. Nevertheless, whatever its source, this idea is behind predicting the future.

The idea of mechanical laws or rules of human action is closely interlinked with the origins of the social sciences, and not just economics. In the case of economics, the driving force behind human action is that of rational choice. Economics is founded on the assumption that individual agents will act in such a way as to maximise their well-being. Together with other axiomatic statements about the nature of tastes and preferences (e.g. they are stable and known) economics then proceeds to deduce theorems about the world. A useful analogy is with geometry. Geometry works forward from a few axioms to make a set of theorems about the world, and these theorems have implications which can be tested. In the case of Euclidean geometry, it turned out that the fifth postulate (the one about parallel lines never meeting) could not be proved and that the alternative Reimann geometry is a more useful description of the

world (at least when the world is understood from the perspective of Einstein's theory of general relativity). Economics is very similar, at least in spirit. Theorems are deduced from axioms and the implications of these theorems can be tested against empirical data. It is here that econometrics is so important. Econometric testing provides the means by which the veracity of the relationships deduced from economic theory can be assessed. The plausibility of macro-econometric theory is thus crucially related to the results of this econometric testing i.e. to macro-econometric modelling.

However, there is an additional condition which must be met if macro-econometric models are to be used for forecasting purposes. Not only must the relationships postulated by the theory be supported to the appropriate degree of accuracy, these relationships must be stable over time[2]. In other words, what has been shown to be the case in the past must continue to be so in the future if forecasts based on extrapolation are to remain plausible. Whilst this is a innocuous assumption in the physical sciences it is not clear that it makes sense in the context of social sciences[3]. The importance of Keynes in this context is that he claims that this assumption is false[4] and there is thus a long standing and deep seated scepticism within economics about the claims which can plausibly made for macro-econometric forecasting. This chapter takes up this scepticism in several different ways and shows that macro-econometric modelling's scientific appearance is little more than skin deep. The search for precise and stable quantitative relationships with which to forecast the future is, as Keynes and others have recognised, a search for a Holy Grail.

The chapter consists of three major sections. The first section introduces the basic accounting and theoretical ideas which underpin the specification of econometric equations and models. The second section examines the stability of one of the equations which make up a macro-econometric model. The method I have used in this chapter is to replicate some econometric work done by Giles Keating[5] in the early 1980s and examine how the equations he estimated using data up to 1980 have been altered by the events of the following 12 years. The analysis shows not just the sensitivity of econometrics to the sample period used but, more importantly from a sociological perspective, the ways in which economists' judgement is routinely used to over-ride econometric evidence which does not conform to the theory. The final section assesses whether a commitment to economic theory improves economic modellers' ability to forecast the economy by comparing the forecasting performance of three different consumption functions.

National Income Accounting and IS-LM Models

In this section I outline the basic accounting relationships within which national economies are discussed and introduce the theoretical framework which underpins the majority of macro-econometric modelling.

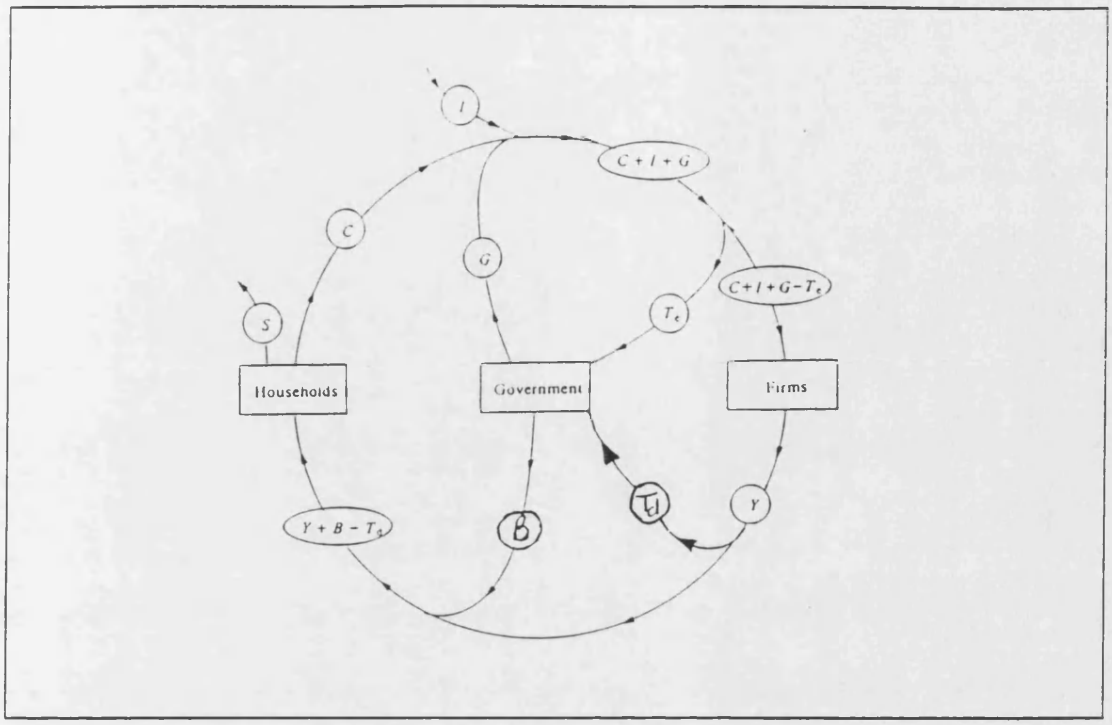
National Income Accounting

In order to keep their analysis and models to manageable proportions macro-economists are forced to aggregate large quantities of information together. This means that the building blocks of micro-economics with which most people are familiar (e.g. the idea of individual agents maximising their utility through rational choices etc.) are no longer appropriate. In macro-economics the basic building blocks are flows of income and expenditure from one sector of the economy to another and the stocks of assets held by each sector. However, this is not to say that macro- and micro- economics are not compatible. All macro-economics should be consistent with rational behaviour and thus the two disciplines complement rather than contradict each other.

The framework of relationships within which macro-economic modelling and policy are discussed and analysed is the National Income Accounting system, which was originally developed by Keynes in the 1930s. This system is outlined below, and begins by considering a simple hypothetical economy which consists only of firms and households. In such an economy, the households use income received in return for labour to purchase goods produced by the firms. If the economy is in equilibrium then the flow of expenditure from firms to households (i.e. wages etc.) is matched by the flow of expenditure from households to firms. This is the basis of the *Circular Flow of Payments*.

The economy shown overleaf in Figure 1 is an extension of this idea. The principle difference is that a tax-levying government sector has also been included. The government is assumed to use the taxes it raises to supply public services and also to re-distribute income. Starting from the right hand side of the diagram, and following the flow of payments around the loop, we can see that firms have a flow of expenditure (Y) called factor payments. These factor payments are made up of wages, dividends and so on. Direct taxes (T_d) are deducted from these factor payments and received as income by the government. Some of this tax revenue is for re-distribution as benefits (B) which are received as income by households.

Figure 1: Flows of Income and Expenditure in a simple economy with tax-raising government.[6]

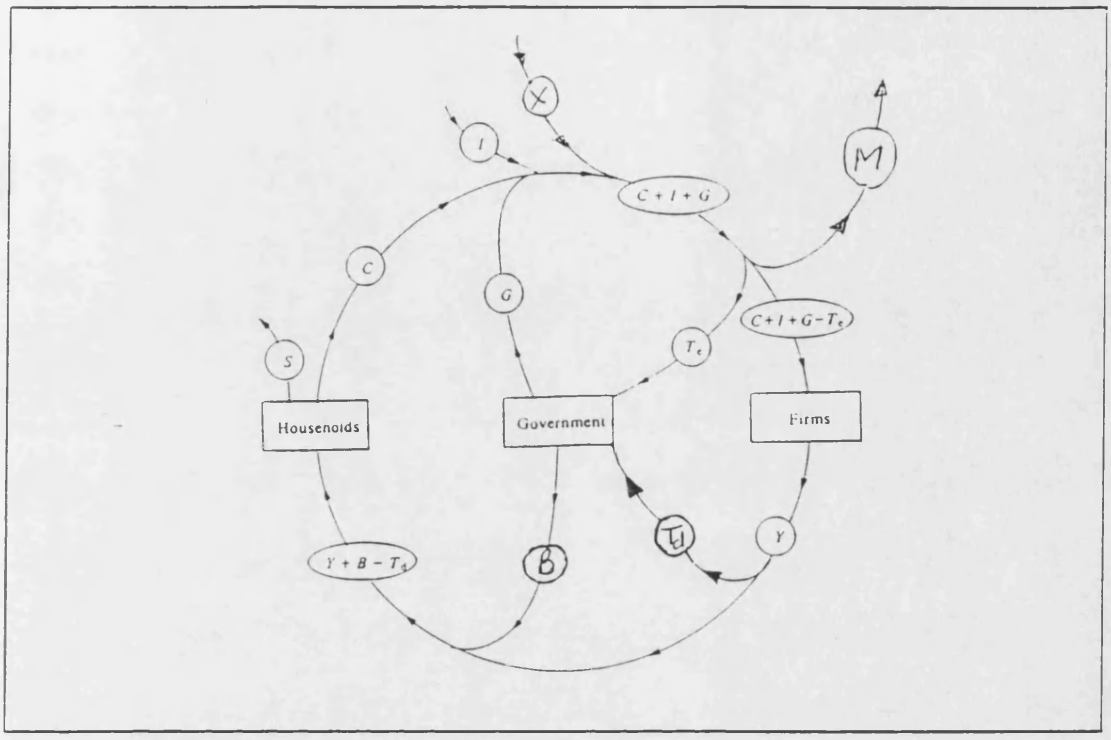


The total income thus received by households is therefore factor payments (Y) minus direct taxes (T_d) plus benefits (B): ($Y - T_d + B$). Households will use some of this income to finance their planned expenditure (C) and will also retain some in the form of saving (S). As savings represent a deduction from households' possible expenditure they are said to represent a leakage from the circular flow of payments; it is only households' expenditure which finds its way back to firms as income. Note also that because many categories of expenditure are subject to indirect taxation (T_e) not all of the households expenditure is received as income by firms. A proportion of households' expenditure will be received as income by the government and used to finance government expenditure programs. Finally, even in this simple economy, households are not the only sector which can contribute to the circular flow of payments. Government expenditure (G) also contributes to the flow of payments as does the expenditure by firms on investment goods such as new machinery and buildings (I).

However, it should be noted that the economy as described thus far has no trading links with any other economy. If international trade were to take place, then exports would be represented as an injection into the flow of payments, i.e. a flow of income into the domestic economy from the rest of the world. Imports, on the other hand, would be represented as a leakage, i.e. a flow of expenditure from the domestic

economy to the international economies. The relationships are summarised in Figure 2. below.

Figure 2: Circular Flow of Payments in a Simple Economy with International Trade



By following the flow of payments around Figure 2, the following definition for National Income (Y) can be derived:

$$Y = C + I + G + X - M$$

The identity defines National Income as the sum of its parts and describes how the flows of income and expenditure within an economy are interrelated. However, it tells us nothing about how National Income is affected by changes in the economic environment, nor does it have anything to say about what determines the level of aggregate expenditure or its components. It is thus only part of the story. In order to fully explain the *level* of National Income, we need to have a theory which links the components of aggregate expenditure to the things which influence the expenditure decisions of economic agents, e.g. to interest rates and money. This is what IS-LM analysis provides.

The IS-LM Model

The theoretical starting point for almost all macro-econometric models is the IS-LM model. As this primarily deals with the demand side determinants of the economy the IS-LM framework is frequently augmented by adding equations such as the Phillips

Curve (which relates to a trade-off between wages and unemployment) to represent the supply side forces which also act on the economy.

In the simple case of the closed economy shown in Figure 1 (i.e. an economy with no foreign trade) there are two basic markets: the market for goods and the market for money. The IS-LM model provides a framework which can be used to analyse the conditions affecting supply and demand in each of these markets. If the theory can be related to economic data, it can then be used to identify the unique combination of National Income and interest rates at which both the money and goods markets will be in equilibrium at the same time.

Equilibrium in the Money Market

For the money market to be in equilibrium the demand for money (L) must equal the supply of money (M). If the supply of money is determined exogenously (e.g. by the government), then the economic theory or model must explain the demand for money. According to Keynes, the demand for money has two main components. Firstly, there is the demand for active balances which are used to finance transactions and to cover unforeseen expenditure. Active balances are thus an increasing function of income. Secondly, there is the demand for idle balances. Idle balances are held by speculative investors and are inversely related to the rate of interest. The total demand for money is thus the sum of the demands for active and idle balances.

What then happens to the demand for money as the level of income increases? If income increases then the demand for active balances will also increase (as more money is needed to finance the increased level of transactions). However, because the supply of money has been fixed by the government, any increase in the demand for active balances must be offset by a decrease in the demand for idle balances. For this to happen, interest rates will have to rise. In other words, for a fixed money supply, increases in the level of income will be met by increases in the rate of interest. Because of this, the LM curve, which shows the combinations of interest rates and income which are compatible with a given supply of money, must have a positive slope.

Equilibrium in the Goods Market

The IS curve defines the conditions for equilibrium in the goods market and is concerned with the relationship between Aggregate Demand and Income. In a closed economy there are two sources of demand for goods: demand for consumption goods by households and demand for investment goods (e.g. machines, buildings etc.) by firms.

Equilibrium in the goods market therefore occurs when the *planned* expenditure of firms and households (i.e. Aggregate Demand) equals their *actual* income. In other words, the goods market is in equilibrium when Income (Y) equals planned investment (I) plus planned consumption (C):

$$Y = C + I$$

By re-arranging an alternative formulation is obtained in which the goods market will be in equilibrium when planned investment equals actual income minus planned consumption:

$$I = Y - C$$

If we now note that savings (S) are, by definition, that part of income not devoted to consumption (i.e. S = Y - C) then the equilibrium condition for the goods market becomes:

$$I = S$$

In other words, the market for goods will be in equilibrium when planned investment equals planned savings. But what happens to investment and savings when the level of income rises? Because economic agents are assumed to always save a certain proportion of income, as income rises, savings rise too. This means that if equilibrium is to be restored in the goods market, investment must also be induced to rise. In the closed economy, planned investment is inversely related to the rate of interest, so the rate of interest must be lowered to increase investment so that it once more equals savings. In other words, the IS curve, which is the set of combinations of income and interest rates for which the goods market will be in equilibrium, has a negative slope.

The IS-LM model

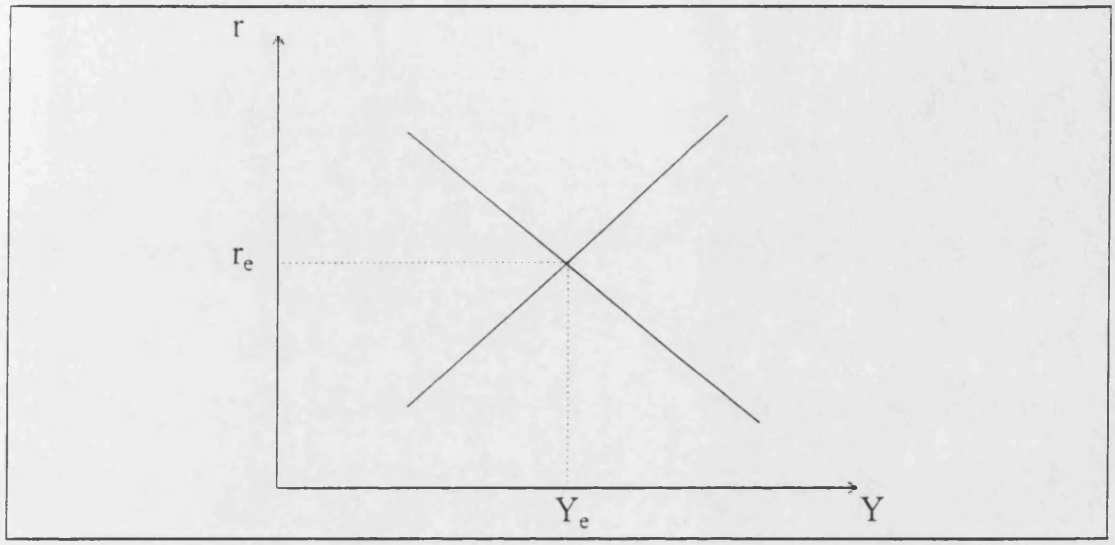
If the IS curve represents a series of equilibria for the goods market in a closed economy, then the LM curve represents the same for the money market. When the two curves are plotted on the same axes, as shown in Figure 3, the intersection gives the unique combination of income (Y) and the rate of interest (r) for which both the goods market and money market will be in equilibrium. The intersection of the IS and LM curves is thus the equilibrium point for the economy as a whole.

Although relatively simple, the basic IS-LM model can be readily expanded to include extra details and 'real world' features. For example, the effect of government

spending (G) and tax raising (T) can be included in the specification of the IS curve, with the effect that the equilibrium condition becomes:

$$S + T = I + G$$

Figure 3: Simple IS-LM model



If this is done, the government is therefore able influence both the LM curve (through its control of the money supply) and the IS curve through its control of fiscal policy and spending programs.

In order to make the IS-LM model suitable for analysing an economy with foreign trade, imports and exports are incorporated into the IS curve in a similar way to the government sector. For the LM curve, exchange rates and the balance of payments have important effects on the money supply. However, these are usually written as separate equations, which at equilibrium must pass through the same point in the (Y,r) plane as the intersection of the IS-LM curves. They thus take the form of extra conditions which must be fulfilled for an overall economic equilibrium.

The Need for Econometrics

Econometrics is the application of statistical techniques to the empirical questions of economics. For example, although economic theory predicts that households' expenditure will be less than their income (i.e. the marginal propensity to consume is less than one), the exact proportion of income which is saved and which 'leaks' out of the circular flow of payments is not specified. Now this might actually be a very important thing to know. For example, a government cutting taxation in the hope of boosting consumption would surely like to know how much of the extra income made

available to households will actually be used to finance expenditure. In other words, the government will want to know what will happen to GDP as a result of its policy decisions.

The value of econometrics is that statistical techniques (principally regression analysis) can be used to obtain quantitative estimates of just these sorts of things. Having derived estimates for, say, the marginal propensity to consume, the econometrician is then in a position to advise the government how much additional consumption will result from the additional income released by the tax reduction. In essence this is what policy analysis using a macro-econometric model is all about. By first modelling the economy and then running simulations it is hoped that the effects of economic policy decisions can be estimated and policies refined so that they have the best possible chance of achieving their targets.

How is this forecasting done?

According to a popular textbook on econometrics[8] there are three basic ways in which econometric modelling and forecasting can be done. The first and most simple way is to predict future values by extrapolating from past values of the variable. Although this technique is not much use for detailed policy analysis, it is a useful way of generating short-term forecasts for time-series data about which theory is either undeveloped or simply not needed. The second technique is to use a single regression equation but include several different independent variables. Thus, for example, in the case of consumer's expenditure (C), we may hypothesise that this is a function of income (Y), and test this by estimating the following regression equation

$$C = \kappa + \alpha Y$$

This single equation approach is perfectly satisfactory when we are only interested in one variable. However, economic policy is usually concerned with a range of variables (e.g. economic growth, inflation, unemployment, public borrowing, balance of trade and interest rates to name but a few). Although separate equations could be used for each policy target this would not reflect the way in which changes in one variable are related to changes in the others. As a result, the third, and most common approach amongst macro-econometric modellers is to use multi- or simultaneous equation models which do represent the inter-relationships within the economy. Thus, a simple model of a closed economy might include a equation for consumers' expenditure, one for

investment and an identity defining National Income. An example of such a model is shown below in Box 1

Box 1: A Simple Model of a Closed Economy

GDP Identity	$Y_t = C_t + I_t + G_t$
Consumption	$C_t = \alpha_1 Y_t + \alpha_2 C_{t-1} + \varepsilon_{\alpha t}$
Investment	$I_t = \beta_1(Y_t - Y_{t-1}) + \beta_1 R_t + \varepsilon_{\beta t}$

Symbols	*Meanings*
Y	National Income or GDP
C	Consumers' Expenditure
I	Investment
G	Government Expenditure (not determined by the model)
R	Interest Rates (not determined by the model).
ε	Error Term

As can be seen, the model in Box 1 defines Income (Y) as being the sum of its parts. Consumption (C) is related to the level of Income and also to its previous values. Investment (I) is related to the change in Income and also to the rate of interest (R). Note that in each case the level of income both determines C and I but is also determined by them through the GDP Identity.

By using powerful computers and sophisticated regression techniques it is possible to estimate large numbers of such equations, disaggregate them, expand them and combine them into highly complex models of the economy. Typically a macro-econometric model uses about five hundred variables and has approximately one hundred estimated equations plus an unspecified number of accounting identities[9]. The next two sections examine these equations in more detail by working through the testing procedures by which a single econometric equation (a consumption function) is specified and tested.

Replicating Econometrics

This section is based on the econometric model described by Giles Keating in his 1985 book *The Production and Use of Economic Forecasts*[10]. It should be made clear at the outset that my intention is not to argue that Keating's method is in anyway inadequate. In fact, my intention is quite the opposite. Giles Keating is undoubtedly a competent and skilled econometrician, and this is precisely why it is worth paying close intention to what he does. My aim is simply to use Keating's method as a guide to 'good practice' and his results as a means of calibrating my own achievements - if I get broadly the same answers as Keating then I can feel reassured that I am on the right

track, radically different results imply a mistake. Of course, if Keynes's pessimism is well founded then some differences are to be expected. Thus what counts as the 'same' is not obvious *a priori* and, as we shall see, the competence of my replication is, inevitably, negotiable[11].

In what follows I describe what I discovered and learnt as I followed the econometric methods set out by Keating. The section illustrates one of the central themes in the sociology of science: the nature of knowledge transfer. The sociology of scientific knowledge argues that knowledge is transferred by means of socialisation into a particular form of life. In a previous paper[12] I discussed in detail some examples of the tacit knowledge macro-economic modellers require in order to succeed and I do not intend to discuss this aspect of the enculturation model of knowledge transfer in any great detail.

The relevance of the enculturation model in this context is the assertion that rules do not contain the rules for their own application. The appropriate way to proceed in science, as in other settings, has to be learnt through participation and socialisation and is not something which can be encoded in a set of rules. With regard to quantitative measurement, surely the most straight forward aspect of science to convey through rules, Thomas Kuhn[13] has shown that even this depends on 'reasonable agreement', the criteria for which are to be found not in the canonical works of scientists, but in their everyday practices. Thus, in some settings, accuracy is measured to six or more decimal places, in others an order of magnitude will suffice. It is the sensitivity to these nuances of context which mark out expertise, not the rote learning of prescriptive methodologies. In economics, I will argue, the actions which follow from statistical significance testing are particularly sensitive to context and interpretation.

This section, and most of the forecasting discussed later, is based on the economic model shown in Box 2 (overleaf). The model is described in detail by Keating in his book. Briefly, from the equations in Box 2 we can see that, the demand for money (the first equation) is related to the level of income and interest rates. It will rise as GDPE increases and people need more cash to finance the increased number of transactions and will fall as interest rates, and hence the cost of holding cash, rise (i.e. the LM curve). The second equation, which is the IS curve, shows that output will tend to fall when real interest rates are high and when domestic prices are high relative to world prices, (i.e. when imports are relatively cheap). The third equation relates

domestic prices to their own past values but also to world prices which means that domestic prices will eventually be brought into line with world prices. Finally, the fourth equation, which is based on uncovered interest rate parity, suggests that any expected appreciation in the domestic currency must be matched by domestic interest rates being below world interest rate by the same amount.

Box 2: A simple linear in logs model of an open economy

LM curve	$\log(MO_t/PC_t) = \alpha_1 \log(GDPE_t) - \alpha_2 RLB_t + \alpha_3 + \varepsilon_{\alpha t}$
IS curve	$\log(GDPE_t) = -\beta_1[(RLB_t/400) - \log(PC_t/PC_{t-1})] - \beta_2[\log(PC_t) - (\log(WWPI_t) - \log(EER_t) + \beta_3 + \varepsilon_{\beta t}$
UK prices	$\log(PC_t) = \gamma_1 \log(PC_{t-1}) + \gamma_2[\log(WWPI_t) - \log(EER_t)] + \varepsilon_{\gamma t}$
Exchange Rate	$\log(EER_t) = \log(XEER_t) + (RLB_t - RSW_t)/400$

Symbols	*Meanings (variables are endogenous unless otherwise stated)*
MO_t	Money supply (MO), exogenous policy variable
$GDPE_t$	Gross Domestic Product, Expenditure measure
RLB_t	UK short term interest rates
PC_t	Domestic Price Index
$WWPI_t$	World Price Index (exogenous)
EER_t	UK exchange rate (foreign currency per unit of sterling)
RSW_t	World short term interest rates (exogenous)
$XEER_t$	Expectations of the exchange rate in the next time period
ε	Error term

From Box 2 it can be seen that Keating's model is essentially a demand side model. By this it is meant that supply-side factors relating to the workforce etc. do not appear. This is not particularly important in this context as these could be included by re-specifying the IS curve to include variables representing the working population or productive capacity and so on. However, the specification of the model is not what makes Keating's book interesting to me, so I am neither going to defend nor criticise the model used. As far as I am concerned it is simply an example of an econometric model. Rather, the interesting thing about Keating's book is that it is primarily a practical book. It is intended to equip the reader with sufficient skills and information to produce their own economic forecasts. The book contains full listings of all the equations used in Keating's model as well as detailed references to data sources and even some blank tables for readers to photocopy and use when preparing their own forecasts. It also describes in considerable detail the methods by which an econometric model is specified and illustrates this discussion with a series of highly detailed examples. It is therefore an excellent starting point for a sociologist (or anyone else) wishing to examine the

theory and methods upon which econometric equations and models are based[14]. My own purpose in following Keating's methods, is to show how a commitment to a particular theoretical understanding of the economy guides Keating's actions and consequently determines the final structure of the model.

In addition to its value as an exposition of econometric methodology, Keating's book can also be used to shed light on the more fundamental question concerning the stability of econometric relationships. By collecting the data identified by Keating and repeating his analysis one can see how the additional data which has become available since 1980 has influenced the regression equations described in the book. This chapter thus also directly addresses the very possibility of an econometric modelling science. If econometric relationships are not stable over time then extrapolating forward from estimated relationships, no matter how statistically sophisticated the techniques used, is going to be difficult to justify.

The Stability of Quantitative Relationships in Econometric Models

In order to replicate Keating's econometrics it was first necessary to obtain the appropriate economic data. As mentioned, Keating provides unusually detailed references and I was able to download most of the data directly from the CSO databank held at the ESRC Archive. The remaining data series, listed in *the National Institute Economic Review*, the London Business School *Economic Outlook* and the OECD *Main Economic Indicators* were entered manually[15]. Finally several transformations of certain series were necessary in order to bring the data in line with that used by Keating. For example, the CSO databank does not include non-oil imports and exports at constant prices, and so this series had to be constructed by transforming the series in current prices which was available.

Once this had been done, I was able to begin estimating the equations given by Keating. The most detailed exposition occurs in chapter 4, where Keating illustrates how one might test the specification of a particular equation, using the consumption function as an example. This section describes in some detail how I worked through the analysis outlined by Keating and examines how closely I was able to reproduce his results. It therefore focuses on the estimation of one of the most fundamental of econometric equations - the consumption function.

Estimating a consumption function

The most general form of Keating's consumption function is given below in Equation 1. It uses 12 explanatory variables plus a constant term. The variables used are the first, second and fourth lags of Consumers' Expenditure (C), Real Personal Disposable Income (RPDI), short term interest rates (RLB) and the rate of inflation (given by the change in prices (PC) plus trend real growth)[16].

Equation 1: General Consumption Function

$$\begin{aligned}\log(C_t) = {} & \alpha_1 \log(C_{t-1}) + \alpha_2 \log(C_{t-2}) + \alpha_3 \log(C_{t-4}) + \\ & \beta_1 \log(RPDI_{t-1}) + \beta_2 \log(RPDI_{t-2}) + \beta_3 \log(RPDI_{t-4}) + \\ & \gamma_1 RLB_{t-1} + \gamma_2 RLB_{t-2} + \gamma_3 RLB_{t-4} + \\ & \varphi_1 [\log(PC_{t-1}/ PC_{t-2}) + 0.0057] + \varphi_2 [\log(PC_{t-2}/ PC_{t-3}) + 0.0057] + \\ & \varphi_1 [\log(PC_{t-4}/ PC_{t-5}) + 0.0057] + \kappa\end{aligned}$$

Using data from 1955Q1 to 1992Q4, I used the regression software RATS to estimate my own coefficients for this formula. Table 1 shows Keating's coefficients (obtained from data running from 1956Q4 to 1980Q4) and mine, together with their appropriate *t* statistics[17]. Before comparing results several caveats should be noted. Firstly, Keating's estimates are derived using data based on 1980 prices and mine use data based on 1990 prices. If re-basing was all that had happened the coefficients should be exactly the same, with the exception of the constant term. However, economic time series are not only re-based but subject to frequent revisions which can literally re-write history. This is particularly important in econometrics and it is not uncommon to find that major papers can be sensitive not just to the sample period used, but also to the date at which the data was published. Thus different vintages of ostensibly the same data set can be sufficiently different for a hypothesis which was supported by one to be rejected by the other[18]. In other words, because of the potential effect of revisions we cannot expect any of the coefficients to be exactly the same and it is possible that even expecting the signs and statistical significance to be unaltered is also unwarranted.

As expected the numerical values differ, although the first two lags of consumers' expenditure do remain highly significant. However, there are differences in both the signs and the significance of other variables. For example, the fourth lag of consumers' expenditure is, for my data, both negative and significant, although only just. Keating on the other hand finds the fourth lag to be positive but statistically insignificant. Similarly, Real Personal Disposable Income is significant for my data,

but not Keating's. Although other differences are apparent (e.g. the sign on the second lags of interest rates and prices) none of these coefficients are statistically significant. What this actually tells us is that neither Keating nor myself are able to reject the hypothesis that the coefficients are actually zero. As such, the differences in sign are quite meaningless.

Table 1: Unrestricted regression coefficients

Variable	Keating Coefficient	Keating t statistic	Evans Coefficient	Evans t statistic
$\log(C_{t-1})$	0.614	5.13*	0.724621821	6.7933*
$\log(C_{t-2})$	0.305	2.29*	0.415723533	3.3044*
$\log(C_{t-4})$	0.0687	0.55	-0.194442601	-2.0436*
$\log(RPDI_{t-1})$	0.170	1.80	0.213586757	2.6352*
$\log(RPDI_{t-2})$	-0.0867	-0.82	-0.214166163	-2.3921*
$\log(RPDI_{t-4})$	-0.0465	-0.57	0.067253889	0.8584
RLB_{t-1}	-0.00340	-2.28*	-0.001567148	-1.5490
RLB_{t-2}	0.00127	0.73	-0.000540274	-0.4553
RLB_{t-4}	0.00015	0.13	0.000536805	0.6598
$\log(PC_{t-1}/ PC_{t-2}) + 0.0057$	-0.250	-0.93	-0.077706992	-0.5042
$\log(PC_{t-2}/ PC_{t-3}) + 0.0057$	-0.0239	-0.09	0.049429281	0.3194
$\log(PC_{t-4}/ PC_{t-5}) + 0.0057$	0.049	0.24	-0.022577034	-0.1774
κ	-0.224	-1.25	-0.124365445	-1.1265

** = statistically significant at 5% level*

The data in Table 1 thus indicates that previous levels of consumption remain an important influence on consumers' expenditure decisions and that the weight attached to these factors has increased. In addition, Real Personal Disposable Income now has a statistically significant effect of consumers' expenditure decisions. However, the rate of interest and the price level remain statistically insignificant, a result which appears to run contrary to economic theory. Of course it is possible that the differences which can be seen between my estimates and Keating's reflect not a change in economic behaviour during the 1980s (and hence the econometric equations) but a difference between the datasets. One way to check this would be to restrict my data set to match the sample period used by Keating and re-estimate the parameters. If the difference is due to the changes in the pre-1980 dataset then this equation should be broadly similar to the one estimated on the full dataset. If however the differences are due to changes in economic behaviour which have occurred since 1980, then the new equation should be broadly similar to Keating's[19]. Unfortunately, as was pointed out in note 17, not all the

data used extended back to 1955 and so comparisons with Keating's equation, although interesting, cannot be conclusive. On the other hand, and perhaps more importantly, comparisons with the parameter estimates detailed in Table 1 will be possible.

Table 2 shows the same equation as in Table 1, but estimated over two different sample periods. Columns 2 and 3 contain parameter estimates obtained when using the full dataset, columns 4 and 5 parameter estimates based on a sample restricted from 1956Q4 to 1980Q4[20] (the same sample period as used by Keating). As can be seen, the coefficients and *t* statistics are much closer to those reported by Keating. For example, Real Personal Disposable Income is no longer statistically significant and, although, some differences between the statistically insignificant coefficients persists, the most important point is that we remain unable to reject the hypothesis that they are zero.

Table 2: Unrestricted regression using full data set and from 1956Q4 to 1980Q4

Variable	**Coefficient (1955Q1 to 1992Q4)**	**t statistic**	**Coefficient (1956Q4 to 1980Q4)**	**t statistic**
$\log(C_{t-1})$	0.724621821	6.7933*	0.364769308	1.8805*
$\log(C_{t-2})$	0.415723533	3.3044*	0.391825177	2.0051*
$\log(C_{t-4})$	-0.194442601	-2.0436*	0.071760777	0.3945
$\log(RPDI_{t-1})$	0.213586757	2.6352*	0.281349618	1.9192
$\log(RPDI_{t-2})$	-0.214166163	-2.3921*	-0.080405825	-0.4957
$\log(RPDI_{t-4})$	0.067253889	0.8584	0.039305984	0.2914
RLB_{t-1}	-0.001567148	-1.5490	-0.002383106	-1.2559
RLB_{t-2}	-0.000540274	-0.4553	-0.002058504	-1.0140
RLB_{t-4}	0.000536805	0.6598	0.000026294	0.0165
$\log(PC_{t-1}/ PC_{t-2}) + 0.0057$	-0.077706992	-0.5042	-0.216661697	-0.8286
$\log(PC_{t-2}/ PC_{t-3}) + 0.0057$	0.049429281	0.3194	0.066755344	0.2377
$\log(PC_{t-1}/ PC_{t-2}) + 0.0057$	-0.022577034	-0.1774	-0.152298981	-0.7159
κ	-0.124365445	-1.1265	-0.715608347	-1.4401

* = *statistically significant at 5% level*

Although the comparisons with Keating's estimates do not resolve the question of whether my estimates are different because of different data or changing behaviour, strong evidence for the latter can be drawn from Table 2. However, by far the most dramatic and interesting thing about the coefficients shown in Table 2 are the differences which emerge as the *same* set of variables are regressed over *different* sample periods within the *same* dataset. In particular, four independent variables which were not significant in the past have become so with the additional data. From having

only one statistically significant variable for data up to 1980, the regression equation based on the full sample has five significant variables. The differences which emerge between columns 2 and 4 illustrate quite clearly that the precise and stable quantitative relationships (which are a pre-requisite for econometric forecasting to make sense) do not exist. The consumption function, which is one of the most important equations in a macro-econometric model, simply does not appear to be stable over time.

However, it is possible to argue that macro-econometric models are used not simply for forecasting but to test economic theory. Indeed, there are some economists who believe that forecasting is inappropriate because it distracts macro-econometric modellers from the much more important task of using the models to test and develop economic theory[21]. From this point of view, it is possible to argue the actual parameter values are not so important. What matters is whether or not a particular coefficient turns out to be significant or not. In the next section I outline how this more theoretical type of analysis may be carried out. I show how the prior commitment to an economic theory is a guiding principle in the estimation of an econometric equation and that the econometric evidence itself seems to be of little importance. At the end of the chapter we return to forecasting and assess the importance of this theoretical commitment by comparing forecasts produced using Keating's theory-derived equation with ones produced using an equation generated by a simple data driven rule.

Econometrics and the Experimenter's Regress

After estimating the first, general regression equation (Equation 1), Keating then proceeds to test a series of restrictions in order to construct a compact and theoretically coherent consumption function for inclusion in his macro-econometric forecasting model. In this section I work through his analysis and argue that it illustrates a well known sociological phenomenon known as the Experimenter's Regress[22]. According to the Experimenters Regress, an experiment can only be successful when it yields the correct results. However, the correct results can only be obtained from competent experiments. When the appropriate answer is well known, the Regress disappears since it is obvious when you have made a mistake. It is only when what counts as the correct answer is itself unknown that the situation becomes tricky[23]. I will argue that the potential for change inherent in social systems means that closure is especially problematic in econometric modelling.

In most of what follows the econometrics is being used to test a standard economic theory and so the 'correct' answer is well known. If I was discussing a demand for money function things would certainly be more controversial. Like Joseph Weber's gravity wave experiments, the econometrics of Friedman can only be rejected once you know what the answer should be[24]. However, even within the most conventional of economic theories room for doubt can creep in, as I shall show.

Testing theories with restricted regressions

The basic methodology for hypothesis testing in econometrics is to specify some sort of restriction on the regression equation, re-estimate it and see if the co-efficients have the appropriate test statistics. Typically one would use the theory to specify an equation in such a way as to test the null hypothesis that a particular variable has no effect. If the coefficient turns out to be statistically significant then the null hypothesis is rejected and the alternative, that the independent variable does have an effect on the dependent variable is accepted. As we shall see, this is not how things work out in practice.

The first restriction imposed by Keating is to drop all the fourth lag variables on the grounds that they are not statistically significant. As all the data used is seasonally adjusted this is what one would expect to find. Interestingly, however, he retains the first and second lags of both RPDI and the inflation term, as well as the second lag of the interest rate term (RLB) despite the fact that these are also statistically insignificant at the 5% level. Why is this? No explanation is offered for this decision, although it seems likely that the central importance of prices, income and interest rates in economic theory means that, for the model to be an 'economic' one, these variables have to remain even if they do no apparent work. Note that for my data set the first and second lag of RPDI are statistically significant and their inclusion is not therefore problematic. The inflation and interest rate variables however remain as articles of faith rather than econometrically supported 'facts'.

The abbreviated equation is then re-estimated and the analysis moves on to consider the coefficient on the second lag of inflation rate, which is shown in Table 3[25]. Because this coefficient is both small and statistically insignificant, the second lag of the inflation term is dropped from the equation. Again this is consistent with my own dataset and so I, like Keating, have good grounds for also excluding this variable from the equation. On the other hand, the reasons for still including the statistically

insignificant interest rate terms remain elusive, especially as the value of the coefficient on the second lag is even smaller and also not significant (see Table 4). Although still not articulated by Keating, it seems that prior theoretical commitments to interest rates and inflation means that they must be included in the model, despite the fact that nearly forty years worth of economic data say they should not.[26]

Table 3: Coefficient on Price Level in first restricted regression

Variable	Keating Coefficient	Keating t statistic	Evans Coefficient	Evans t statistic
$\log(PC_{t-2}/\ PC_{t-3}) + 0.0057$	0.0111	-0.05	-0.001311091	-0.0097

After removing the second lag of the inflation rate, the consumption function has as explanatory variables the first and second lags of consumers' expenditure, RPDI and interest rates, as well as the first lag of the inflation rate plus a constant term.

The next stage of specification testing involves re-arranging the consumption function. First the dependent variable is changed from the level of consumers expenditure to its first difference (i.e. $[\log(C_t) - \log(C_{t-1})]$). The explanatory (or independent) variables remain unaltered and so, if all is well, the coefficients should remain the same (after all the equation has not been altered, only re-arranged). The only exception to this should be the coefficient on lagged consumers' expenditure, which should be exactly one less than it was before. The coefficients are shown in Table 4, below, and we can see that this is indeed the case[27].

Table 4: Rearranged Regression Equation (Evans' estimates)

Variable	Dependent Variable = log(C)	*t* statistic	Dependent Variable = $\log(C_t) - \log(C_{t-1})$	*t* statistic
$\log(C_{t-1})$	0.724865368	7.0741*	-0.275134633	-2.6851*
$\log(C_{t-2})$	0.264678161	2.6195*	0.264678162	2.6195*
$\log(RPDI_{t-1})$	0.232378425	3.0648*	0.232378425	3.0648*
$\log(RPDI_{t-2})$	-0.206237816	-2.6044*	-0.206237816	-2.6044*
RLB_{t-1}	-0.001347448	-1.3995	-0.001347448	-1.3995
RLB_{t-2}	-0.000457985	-0.4621	-0.000457985	-0.4621
$\log(PC_{t-1}/\ PC_{t-2}) + 0.0057$	-0.031367560	-0.2851	-0.031367560	-0.2851
κ	-0.151510755	-1.6385	-0.151510755	-1.6385

* = *statistically significant at 5% level*

The next re-arrangement is to replace the second lag of consumers' expenditure by a difference (first lag minus second lag), and the lagged values of RPDI by a

difference and a 'new' variable formed by subtracting income lagged once from expenditure, also lagged once. However, as this re-arrangement of C and RPDI uses only the previously existing independent variables, the coefficients on the interest rate and inflation terms should be unchanged; the others alter so as to offset the effects of the re-arrangement. The results of this regression are shown in Table 5[28].

The purpose of this re-arrangement is to make it possible to test the long run form of the equation. In particular, the long run form of the consumption function should contain all the variables shown in Table 5, apart from the lagged level of consumers' expenditure. If its coefficient is not significant, then this variable can be dropped from future regressions and the equation will have a form which is consistent with economic theory.

Table 5: Regression with further re-arrangement

Variable	Dependent Variable = $\log(C_t) - \log(C_{t-1})$	*t* statistic
$\log(C_{t-1})$	0.015684138	1.8338
$\log(C_{t-1}) - \log(C_{t-2})$	-0.264678162	-2.6195*
$\log(RPDI_{t-1}) - \log(RPDI_{t-2})$	0.206237816	2.6044*
$\log(C_{t-1}) - \log(RPDI_{t-1})$	-0.026140609	-0.3888
RLB_{t-1}	-0.001347448	-1.3995
RLB_{t-2}	-0.000457985	-0.4621
$\log(PC_{t-1}/ PC_{t-2}) + 0.0057$	-0.031367560	-0.2851
κ	-0.151510755	-1.6385

** = statistically significant at 5% level*

As can be seen from Table 5, the *t* statistic is 1.83, which is not significant at the 5% level. The level is therefore excluded and the re-estimated equation is shown in Table 6[29].

Table 6: Long run form of equation

Variable	Dependent Variable = $\log(C_t) - \log(C_{t-1})$	*t* statistic
$\log(C_{t-1}) - \log(C_{t-2})$	-0.271098125	-2.6565*
$\log(RPDI_{t-1}) - \log(RPDI_{t-2})$	0.242976266	3.1383*
$\log(C_{t-1}) - \log(RPDI_{t-1})$	0.020673571	0.3289
RLB_{t-1}	-0.000940766	-0.9936
RLB_{t-2}	0.000146400	0.1550
$\log(PC_{t-1}/ PC_{t-2}) + 0.0057$	-0.090350552	-0.8550
κ	0.017699217	2.9077*

** = statistically significant at 5% level*

In the 'long run' the growth rates of real consumers' spending and income and the levels of interest rates and inflation are all, by assumption, constants. Keating

further assumes that spending and incomes rise at the same rate, and so the constants are, respectively, k, k, r and p. From Table 6, this gives the long run consumption function, Equation 2.

Equation 2: Long run consumption function

$$k = (-0.271 + 0.243)k + 0.021(\log(C) - \log(RPDI)) + (-0.0009 + 0.0015)r - 0.091p + 0.018$$

Since k, r and p are all constants, the equation shows that there is a constant difference between log(C) and log(RPDI). In other words, a constant average propensity to consume. In order to reflect the standard economic theory that proportionally more savings are needed at higher rates of inflation, in order to maintain the real value of savings, this constant should be low at high values of p. For this condition to be satisfied, the sign on [log(C) - log(RPDI)] must be the same as that on p.

This can be seen by re-arranging the function to express the ratio of expenditure to income, as shown below. Equation 3 shows the results based on my parameter estimates, and Equation 4 shows the same equation as presented by Keating.

Equation 3: Average Propensity to Consume, derived from Evans' estimates

$$C/RPDI = \exp.[(1.028k - 0.006r + 0.091p + 0.018)/0.021]$$

Equation 4: Average Propensity to Consume, derived from Keating's estimates

$$C/RPDI = \exp.[(-0.163k - 0.00187r - 0.179p + 0.00960)/0.148]$$

It can be seen that as p increases the right hand side of Equation 3 also increases, as the sign on the co-efficient is positive. This implies that consumers do not raise savings at rates of inflation, but actually save less. This is a situation in which it is clear that there is something wrong with my analysis; my econometrics have produced an incorrect result which would probably be discounted by the majority of economists because it conflicts with the accepted theory. In fact if I were an economist myself I would probably not believe the result and would try changing the variables or the sample period until I had estimated an equation which matched the theory[30]. This of course would be the result I would try to get published!

In fact this is exactly what I did do. Initially I restricted the sample period to the one used by Keating, but found that the although both the coefficients had changed sign they were nevertheless still different. I therefore experimented with a few different sample periods and found that extending the sample period by four observations (i.e. to

run from 1956Q4 to 1981Q4) was sufficient to bring the estimated equation back in line with the conventional theory. Of course, it should be noted that the coefficient on the inflation rate was never statistically significant in either Keating's analysis nor any of my own regressions and so all this could well be regarded as quite pointless. Strictly speaking, what Keating's econometrics and my own actually tell us is that, at the levels of statistical significance conventionally used, the co-efficients on the inflation terms are indistinguishable from zero.

However, it is now clear why Keating chose the particular strategy that he did. The aim of the exercise was to produce a compact and theoretically coherent consumption function, and the theory defined in advance what form this consumption function should have. As a result certain statistically insignificant variables were also economically insignificant and could be discarded. On the other hand, other equally statistically insignificant variables were economically very salient indeed. This latter group of variables had to be retained in order that the equation could be presented in the appropriate form and the reasons for ignoring certain t statistics and respecting others are now clear.

Testing Further Restrictions and Hypotheses

Apart from testing that the long run form of the equation matches the standard theory, Keating also tests a further hypothesis. Keating argues that, in addition to maintaining the real value of asset holdings, consumers will tend to increase these asset holdings to allow for real growth in income. The reason for this is that if they failed to do so then, over time, their holdings of financial assets would become either a very high or very low proportion of their income. On this assumption, the average propensity to consume equals a constant times the sum of the inflation rate and the trend real growth rate and implies some further restrictions on the regression equation. The hypothesis is tested in two stages.

In the first stage, the steady state growth rate (2.3% per annum, or 0.57% per quarter) is subtracted from the dependent variable, and also from the right hand side consumption and income growth variables. The coefficients for this equation are shown in Table 7 below[31]. The first stage of the test is to see if the constant term is significant. If Keating's hypothesis is correct, then the constant should *not* be significant, and this was indeed the case for his equation. (Keating; $\kappa = 0.00295$, $t = 0.72$).

Table 7: Regression with steady state growth

Variable	Dependent Variable = $\log(C_t)$ - $\log(C_{t-1})$ - 0.0057	*t* statistic
$\log(C_{t-1}) - \log(C_{t-2}) - 0.0057$	-0.271098125	-2.6565*
$\log(RPDI_{t-1}) - \log(RPDI_{t-2}) - 0.0057$	0.242976266	3.1383*
$\log(C_{t-1}) - \log(RPDI_{t-1})$	0.020673571	0.3289
RLB_{t-1}	-0.000940766	-0.9936
RLB_{t-2}	0.000146400	0.1550
$\log(PC_{t-1}/ PC_{t-2}) + 0.0057$	-0.090350552	-0.8550
κ	0.011838922	1.9983*

** = statistically significant at 5% level*

However, from Table 7 we can see that the constant term is statistically significant at the 5% level (t = 1.99829). The implication here is that consumers are no longer raising their asset holdings to allow for growth in real income. Arguably this is what happened during the 1980s when consumers, as a group, greatly increased their financial liabilities and thus their assets holdings, relative to income, would have fallen. Indeed, one of the more distinctive features of economic behaviour in the 1980s was the decline in the saving ratio (which fell to about 5% in 1988[32]). Taken together these observations imply that consumers behaviour may well have changed during the 1980s.

It is here that the idea of the Experimenter's Regress is more pertinent as the what counts as the 'correct' answer is not clear. It is at least possible that the behaviour of consumers was actually different in the 1980s and that the statistical significance of the constant term is reflecting this change.[33] Now, this change might either be a once-and-for-all event (i.e. consumers no longer raise assets holdings in line with growth in income) or a temporary 'blip' in behaviour, but it is something which a competent economic evaluation might reasonably be expected to pick up. In other words, this situation is quite different to the long run case discussed above. In this set of circumstances, getting the same result as Keating, rather than challenging my competence as an econometrician, might do quite the reverse. Interestingly, even at the time Keating was writing, the data were not entirely consistent with the hypothesis.

As mentioned above, testing the hypothesis involved two stages. The first was that the constant term in the regression equation which includes the steady state growth rate should not be statistically significant. The second stage involves re-estimating the equation without the constant term and with the second lag of interest rates replaced with a difference. If the restriction is correct, the coefficient on the level of interest rates should be insignificant. The coefficients from Keating's equation and my own are

shown in Table 8 below[34]. As can be seen, the hypothesis that the coefficient on the level of interest rates is zero is rejected at the 5% level for Keating's data set and it should therefore be retained in the equation. However, as Keating points out, a *t* statistic of 2.48 is not statistically significant at the 1% level and so the interest rate variable can be dropped from the equation. In other words, by selectively invoking more stringent criteria, the hypothesis derived from economic theory is supported.

Table 8: Regression with Interest Rate Difference.

Variable	Keating Coefficient	Keating t statistic	Evans Coefficient	Evans t statistic
$\log(C_{t-1}) - \log(C_{t-2}) - 0.0057$	-0.215	-2.21*	-0.191405800	-2.0110*
$\log(RPDI_{t-1}) - \log(RPDI_{t-2}) - 0.0057$	0.0547	0.67	0.210140959	2.7411*
$\log(C_{t-1}) - \log(RPDI_{t-1})$	-0.182	-3.42*	-0.072370013	-1.6914
RLB_{t-1}	0.00186	-2.48*	-0.000585757	-1.3796
$RLB_{t-1} - RLB_{t-2}$	-0.00183	-1.44	-0.000217112	-0.2270
$\log(PC_{t-1}/ PC_{t-2}) + 0.0057$	-0.211	-1.17	-0.090701326	-0.8471

** = statistically significant at 5% level*

For my data, the hypothesis is rejected at the 5% level, though the meaning of this is unclear as, in order to maintain comparability with Keating, I have dropped the constant term even though it was significant. Of course, if I was a believer in the theory, I could adopt a similar strategy to Keating and remain unconvinced by the story about changing consumers' behaviour and falling asset holdings. Instead I too would change the rules. For example, I could look at Table 7 and argue that the *t* statistic for the constant was barely significant at the 5% level ($t \approx 2$) and was certainly not significant at 1% level. Therefore the constant should be excluded on the grounds that it failed to pass the stricter significance test. From Table 8, I would argue that the co-efficient on the interest rate level was also insignificant and that this too should be excluded. In other words, the hypothesis that consumers raise their asset holdings in line with income growth is in fact thoroughly consistent with the data.

This then is how the Experimenter's Regress can manifest itself in econometrics: valid results are only obtained from appropriate econometrics, but the authority of the econometrics is only established by producing the correct results. In order to know how to interpret Table 7 we have to already know what the correct answer is. Should we

exclude the constant or invoke more stringent tests? Have assets become a low proportion of consumers income or not? This is what Table 7 is supposed to be telling us, but without some way of knowing what has happened to asset holdings in the private sector we cannot tell. In other words, simply looking at Table 7 cannot answer our question until there is no question left to ask.

Econometrics and Theory Testing

The value of an econometric model could be that it forces economists to think carefully about what they are doing. Each variable included in an econometric model should be consistent with the wider corpus of economic theory. Ideally each variable would also be supported by the data, although, as we have seen, this latter criterion is only loosely applied. Thus, in principle, the *a priori* specification of the model should survive the process of hypothesis testing and estimation unscathed and an econometrically justified model be produced. In practice, however, a considerable amount of conviction and faith on the part of the economist is needed if the theoretical specification is to be sustained in the face of the econometric evidence.

The previous section showed that one needs a reasonably clear idea about how the economy works in order to specify a model in the first place. Indeed, it seems one needs a strong set of prior commitments largely because the econometric evidence for the theory is so weak. A researcher who believed in the power of statistics, as opposed to the power of economic theory, would probably produce a very different sort of model. The failure of econometrics to support economic theory is doubly ironic as econometric textbooks often refer to the ease with which statistically significant regression equations can be produced. Paradoxically, textbooks frequently argue that economic theory is important because it enables students to sort the 'wheat from the chaff'. Thus, in the introduction to his econometrics software package *PC-Give* David Hendry writes:

> Even at an elementary level a critical appreciation must be engendered, especially for time series econometrics ... Challenge the class to select any two variables ... and you guarantee to produce a correlation of over 0.95 between the two variables after at most one transformation on each variable ... You would be unlucky to lose this challenge.[35]

The implication here is that there is such a profusion of statistically significant relationships that finding support for a theory is all too easy. My own example of

repeating Keating's analysis suggests that this rather overstates the case. In fact, from my account, it looks as though theory is needed not to discriminate between the important and the statistically significant but rescue the important from the statistically insignificant. In other words, rather than separating out the wheat, it might be more accurate to say that economic theory provides a way of sorting through the chaff to find those husks which, by virtue their colour or shape, are deemed desirable.

Of course, it might be argued that Keating is perhaps unusual in the strength of his prior commitments[36] and that his example is not therefore typical of economists or econometricians in general. Certainly this has not been my intention. As emphasised earlier, I am using Keating's book as a guide to good practice. As the status of Keating's econometrics is crucial to the credibility of my own argument it is important to remember that there is nothing unusual about Keating's approach to econometric modelling. On the contrary, Keating's example is remarkably consistent with something I found out very early on in my study of macro-econometric modelling. In my very first interview with an econometrician I learnt that it is considered quite normal to retain statistically insignificant variables if they are theoretically interesting.

Hudson The model builder has to have a theory about what goes into an equation. He doesn't just look for *t* statistics and significance in an equation because that tends to lead to bad forecasts. If you've got something which is statistically significant and makes theoretical sense then you are unlikely to be picking up chance correlations.

Evans So in some cases you would keep in [explanatory variables] that perhaps weren't as statistically significant as you would like

Hudson Often

Evans You would say: I think it should be in there anyway, so I'm going to leave it in even though the *t* statistics say I should drop it.

Hudson Yes

Thus, in contrast to the textbooks and other representations of canonical science, the relationship between evidence and theory is, in practice, very complex. There are circumstances in which statistically significant variables will be discounted because they are not theoretically relevant and other circumstances in which statistically insignificant variables are retained because they are believed to be important. Only

occasionally do we encounter the happy situation in which a theoretically important variable comes accompanied by a significant *t* statistic.

It is because of the ambiguity of within-sample econometric tests that, as pointed out in the quotation above, out-of-sample forecasting is important. The intuition is that any relationship due to chance which might have been found within the sample period will quickly be exposed by out-of-sample testing. Before discussing this process of testing in more detail, it will be useful to outline a typology of possible forecast tests and to introduce some terminology.

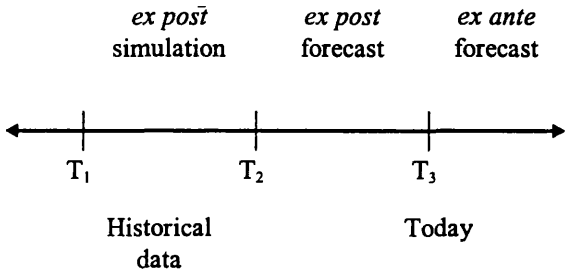
Out-of-sample Testing

A typology of forecasting

Economic models can be made to forecast in a variety of different ways and these methods can be ranked in a hierarchy in which each represents a more difficult challenge than the one which came before. The range of possible tests is illustrated in Figure 4, below.

The first simulation test which can be performed is an *ex post* simulation test in which the model is made to forecast the endogenous (i.e. "left hand side") variables for some portion of the historical period over which it was estimated. *Ex post* simulations can be either static or dynamic. In a static simulation, the lagged endogenous variables take their actual values, whereas in a dynamic simulation, the values generated by the model are used. In Figure 5, which shows forecasts and simulations using several techniques and is based on the different versions of the unrestricted regression equation (Equation 1), the fitted values are calculated using static *ex post* simulations.

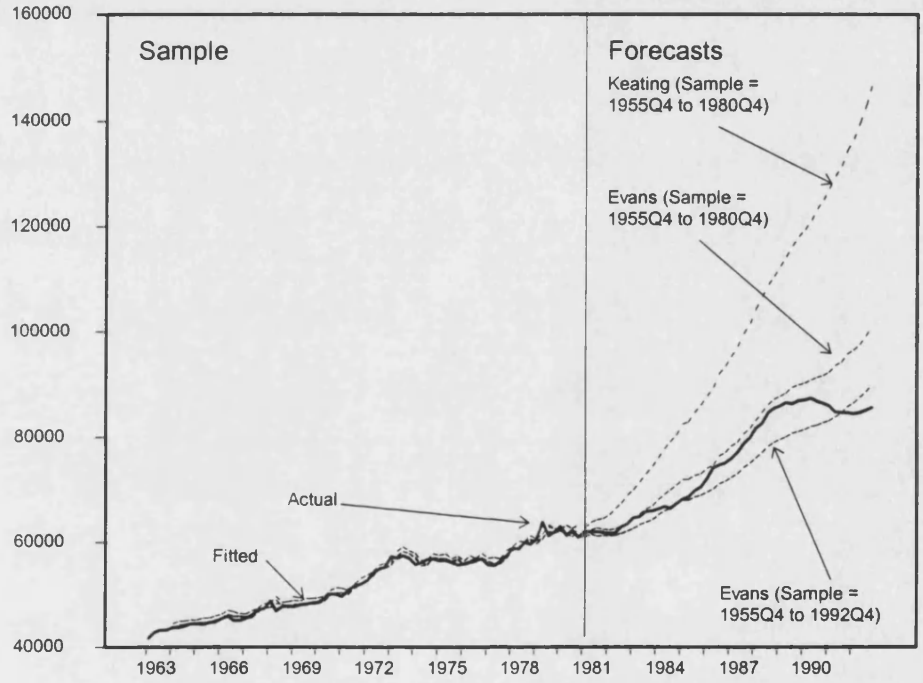
Figure 4: A typology of forecasts[37]



Next in the hierarchy is *ex post* forecasting, in which forecasts are generated for the historical period which lies between the end of the estimation period and the present

day. Like *ex post* simulations, an *ex post* forecast may either be a static (or one-step ahead) forecast or a dynamic forecast. In Figure 5 (below) the forecasts generated by the equations estimated on data up to 1980 are dynamic *ex post* **forecasts**, but the values generated by the model estimated on data up to 1992 are dynamic *ex post* **simulations**. *Ex Post* forecasts are more difficult than simulations, because any information about changes in the economic environment which have occurred after the sample period will not be reflected in the coefficients.

Figure 5: Forecasts of Consumer's Expenditure from 1980 to 1985



The final and most difficult test of a model's usefulness is *ex ante* forecasting, in which forecasts are generated for the future values of the endogenous variables. Unlike dynamic *ex post* forecasting, where at least the values of the exogenous variables are known from the historical data, *ex ante* forecasting requires the forecaster to predict the future values of all variables not explicitly forecast by the model before the forecast can be produced. *Ex ante* forecasting therefore takes both the model and the modeller into the unknown and, in effect, the economist has to 'forecast the policies so that the model can forecast the economy.'[38] *Ex ante* forecasts are not shown in Figure 5.

What does Figure 5 show?

Figure 5 shows *ex post* simulations and forecasts for the period 1981 to 1992 using each of the three equations estimated[39]. The values shown to the left of the vertical line are all static *ex post* simulations. These values were calculated by using the actual value taken by all independent and lagged dependent variables and setting the error term to zero. The dynamic *ex post* forecasts (and simulation for the equation based on data up to 1992) are calculated by again using the actual values for all independent variables and assuming a zero error term, but the lagged values of the dependent variable are obtained by using the value generated by the model in the previous period.

As can be seen, all three regression equation fit the data for the duration of the sample period. During the 1980s however, the fit with the data is significantly worse for all three forms of the equation. Keating's equation forecasts least well, though this may be due to the fact that his measure of short term interest rates is UK banks base rates, whereas mine is the average discount rate on Treasury bills (CSO series code AJNB). This hypothesis could be tested by obtaining a series for UK banks base rates and re-calculating the forecasts for the Keating equation. However, later we shall see that other equations due to Keating do not suffer from this problem, thus suggesting that the cause may be internal to the equation, rather than to the data mismatch[40].

With regard to the two equations based on my own dataset, the apparently large differences in some coefficients do not seem to be reflected in the dynamic forecast tests. Both equations seem relatively close to actual consumers' expenditure for 1981 and the first half of 1982, and the equation based on the restricted dataset fits the observed data until about mid 1983, which is a good forecast by any standards. Thereafter, the equation based on date up to 1980 tends to over predict and the one based on data up to 1992 consistently under predicts. This may well be due to the effect of the downturn in consumers' expenditure which took place at the end of the decade (this information was available during estimation and should therefore be reflected in the coefficients).

Static* ex post *simulations

It is surely a minimum requirement that a model which is claimed to explain a set of data must fit that data reasonably well. In the case of the three consumption functions shown in Figure 5 it is clear that, for the sample period chosen, all three equations are close to the actual value of consumers' expenditure. In fact they are so close together

that the printer is unable to draw them as separate lines. This need not be surprising however, as the fitted values represent functions which minimise the sum of squared deviances from the actual values. One would therefore expect that all functions generated by this method should fit the actual data quite closely, no matter how different their formulas may look. Thus, even though the coefficients are different, perhaps as a result of data revisions and other differences between the datasets, the fit of the lines to the observed values is remarkably similar. The portion of the chart to the left of the vertical line is probably best thought of as a graphical representation of an R^2 of over 98%.

Thus, from Figure 5 we can see that, for the values actually taken by consumers' expenditure, income, interest rates and prices, the differences in the coefficients are not particularly important. This, in turn, helps understand why out-of-sample forecasting and prediction is so important in assessing the merits of an econometric model.

Dynamic* ex post *Forecasts and Simulations

An out-of-sample test is usually done by only using part of the available data to estimate the equations, and then using the model to generate predicted values for the remainder. If the model uses its own forecasts as inputs in all iterations beyond the end of the sample period, then the *ex post* forecast is a dynamic one. Because the model is forecasting values which are already known, there is a 'right' answer (i.e. the actual outcome) against which the performance of the model can be assessed. In Figure 5 this sort of test is only really carried out on versions 1 and 2 of the consumption function (i.e. those estimated on data up to 1980Q4). As noted above, the unrestricted consumption function estimated on my dataset predicts consumers' expenditure rather well until about 1983-4. Keating's function did less well, though this might due to it being 'fed' the wrong measure of interest rates. In addition to the data problem, it seems that for Keating's unrestricted equation, the initial over estimate of consumption for the first forecast period is then compounded in the following iterations, producing a forecast which explodes upwards. This may be independent of the interest term problem.

Turning to the forecast mistakes which appear after 1983-4, these are not unexpected and may be attributed (at the very least) to any or all of the following: the radical economic policies introduced by the Thatcher government; the conflict in the Falklands; the miners' strike. Although other explanations are undoubtedly possible,

the general hypothesis that there is a structural break in the economic time series can be tested by comparing equations estimated on data covering the periods before and after 1981[41].

The third 'forecast' cannot really be compared to the other two in this way as, although dynamic, it is not strictly speaking 'out-of-sample' (the equation was estimated on data up to 1992Q4). The values are thus *ex post* simulations because the coefficients for this equation thus already contain information about the nature of the relationship between the dependent variables during the forecast period. It is as if a forecaster had, in 1980, access to the next 11 years worth of economic data when estimating their model. What is interesting about the simulation produced by consumption function 3 is that it implies that even if forecasters had somehow obtained access to data about the boom and bust in the 1980s they would still have been unable to forecast the rise in consumers' expenditure any more accurately. In fact, it is consumption function 2 which appears to do the best job in anticipating the rise in consumers' expenditure (although it is unlikely that it would have been believed).

However, this may be too harsh. The additional information provided by the extra data is reflected in that consumption function 3 tends to be lower than the one estimated on data up to 1980. The sharp fall in consumers' expenditure which occurred at the end of the 1980s thus tends to pull the equation down. If the mean square errors due to functions 2 and 3 were calculated, it is likely that the relative accuracy towards the end of the forecast period of the function estimated on the full data set would ensure that it had the best performance overall. What the *ex post* simulation produced by function 3 does show, and shows quite clearly, is the importance of using the actual value of lagged dependent variables when calculating the fitted values. The only difference, insofar as function 3 is concerned, between the forecast and fitted values is that the lagged values of consumers' expenditure are not the actual ones, but the ones forecast by the model. It is clear that this change in procedure has had a very important effect on the performance of the model.

Finally, it can be seen from Figure 5 that the consumption function estimated on data up to 1980 predicts fairly well until about 1983, i.e. for about 2 years. After this it breaks down and does not fit so well. However, the early 1980s were a time of radical policy change so maybe this is not surprising. In addition, economists are not concerned purely with prediction, but also with a theoretical understanding. With this in

mind, it is worth examining how the final version of the consumption function derived through the specification testing procedures outlined in Sections 4.1 and 4.2 above performs in out-of- sample testing. The aim in this final section is to assess the value added by using economic theory to guide the specification of an econometric equation.

The Consumption Function and Economic Theory

In Chapter 4 of his book Keating discusses briefly the out-of-sample testing of the consumption function he has derived. By comparing the forecast 10 quarters ahead with the known outturn Keating is able to compute a test statistic (he does not define which one), according to which the hypothesis that forecast values are sufficiently close to the actual values for the errors to be considered random cannot be rejected. However, because the test is based on a comparison of within sample errors to out-of-sample ones it is possible for an equation which fits the historical data very badly to pass the test not because it forecasts well but because the error terms in the past have also been large. Keating checks for this possibility by examining the standard error of the estimated equation which provides an estimate of the square root of the variance of the error terms. In Keating's case this figure is 0.01177 which, because the dependent variable is measured in natural logarithms, implies about 1.2%[42]. As Keating admits:

> There is no formal measure of whether this is a large or small standard error, but a judgement can be made by comparing it with the average change in consumers' expenditure over the estimation period. This suggests that the standard error is neither exceptionally large nor very small, implying that the forecast test is a reasonably good guide to the forecasting ability of the equation[43].

A further point worth noting is that the out-of-sample forecasting testing done by Keating is a static test. In other words, the lagged values of the dependent variables are the actual ones rather than the ones forecast by the equation. We saw in Figure 5 that the difference between static simulations and dynamic simulations was quite significant and that, as Keating also notes, dynamic tests are much harder.

In what follows I compare the out-of-sample forecasting performance of the consumption function used by Keating and specified according the methods outlined above with my own version of the function and a third equation derived according to a simple statistical rule. The rule used was the following: starting from the general consumption function in Equation 1, exclude the variable with the lowest *t* statistic and

re-estimate the equation, repeating this process until all remaining variables are significant at the 5% level[44]. The equation produced is shown in Table 9 below.

Table 9: Equation derived by application of statistical rule

Variable	Dependent Variable = $\log(C_t)$	t statistic
$\log(C_{t-1})$	0.698102447	6.4878*
$\log(RPDI_{t-1})$	0.301617075	2.8206*
RLB_{t-1}	-0.002760012	-3.9310*

* = *statistically significant at 5% level*

The purpose of including this equation was to examine the 'value added' by Keating's economic theory. If economic theory does explain how consumers behave then it seems reasonable to expect that a model based on this theory will prove a better forecasting model than one derived from an atheoretical data fitting methodology, especially over long periods[45].

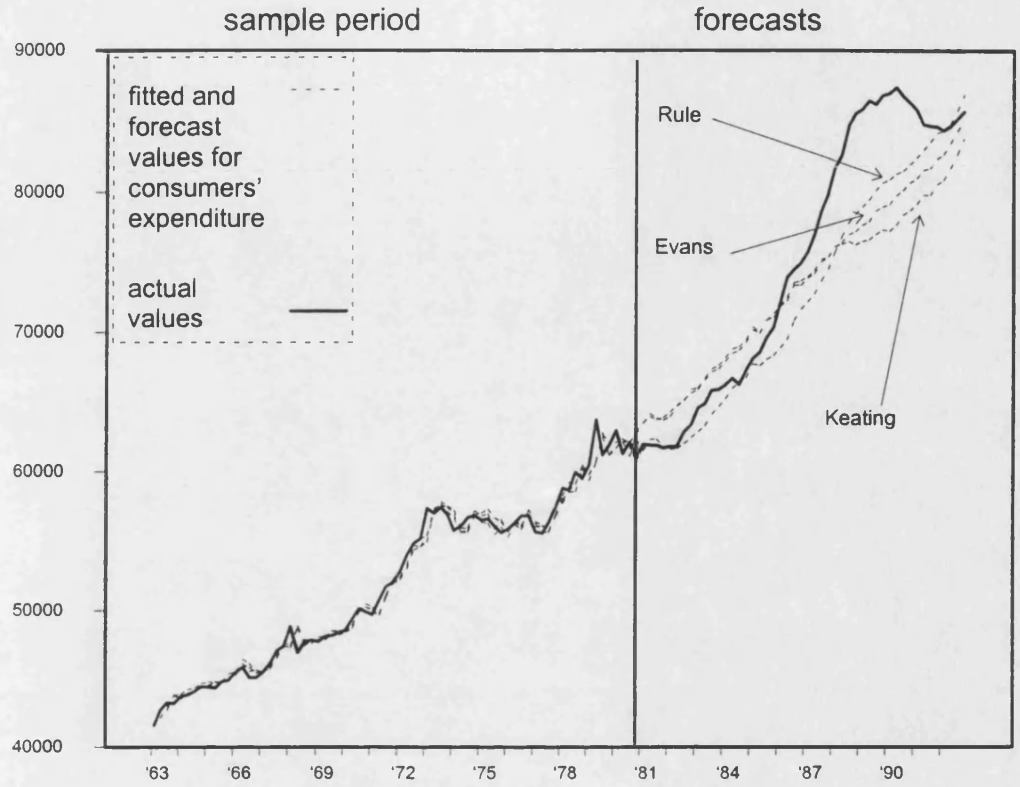
Figure 6 shows fitted values (static *ex post* simulations) and dynamic forecasts for three equations, all of which were estimated on data up to the end of 1980. The lines labelled 'Keating' and 'Evans' show the *ex post* simulations and forecasts for consumers' expenditure, calculated using the specification procedures outlined previously. The line labelled 'Rule' shows *ex post* simulations and forecasts calculated using the equation estimated by the rule: exclude the least significant variable and re-estimate.

As was the case with Figure 5, which showed the values calculated from the general regression equation, the fitted values are very close both to each other and the actual value. However, unlike Figure 5, the equation due to Keating and the one estimated by myself are now very close together and the tendency for them to accelerate explosively, which was particularly marked in Keating's equation, seems to have gone. In addition, it now looks as if the dynamic forecasting performance of the equations is very similar and any differences between the datasets used by Keating and myself do not appear to have had much effect.

Thus, from Figure 6 it appears that the specification search procedure has had the following effects. Firstly, as might have been hoped, it has produced an equation which is robust with regard to the vintage of the data set and the measures of interest rates used. Both equations fit the past data very well and the out-of-sample forecasting performance appears to be similar. Less positively however, the result seems to be that both equations now forecast a more or less constant rate of increase in consumers'

expenditure through out the 1980s, missing both the downturn at the beginning and the boom in the middle. However, by the end of the decade the downturn caused by the recession has brought the actual values back into line with those predicted by the model.

Figure 6: Fitted and Forecast Values for Consumers' Expenditure using equations based on theory and data fitting



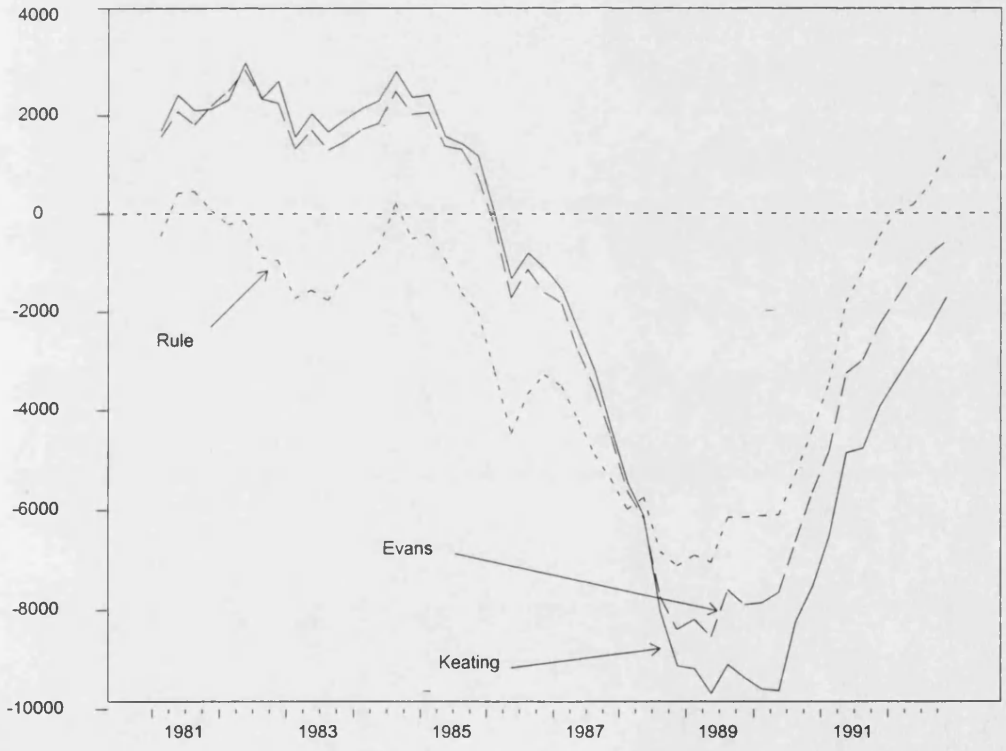
The performance of the equation derived from the data fitting rule is rather different from the equations derived from economic theory. The mechanically derived equation correctly forecasts the downturn, and is thus closer to the actual values of consumers' expenditure during the first few years of the decade. Thereafter it too under-predicts the growth in consumers' expenditure but seems more responsive to changes in its other independent variables (i.e. Real Personal Disposable Income and short term interest rates) and finishes the decade with the highest forecasts. The impression gained from Figure 6 is that the 'Rule' based model tracks the upturn in consumers' expenditure more closely.

Although Figure 6 shows the history of the 1980s it is not necessarily the best way of examining the relative merits of the three equations as forecasting models. To do this it is more useful to look at the errors, and these are shown in Figure 7 (overleaf) in which the story outlined in the previous paragraphs is again visible. The theory based models used by Keating and myself make a similar pattern of mistakes. The over-

predictions at the beginning of the decade are shown in Figure 7 as positive errors, and the under-prediction of the boom years as an increasingly large negative errors. The recession at the end brings forecast and reality back together.

In Figure 7, the forecast of the rule derived equation is close to the actual outcome at the beginning of the decade, and for short term forecasting, at least, is clearly superior to either of the equations derived from economic theory. Again, this is not necessarily an unusual phenomenon. For example, Wallis and Whitely[46] found that over short term forecasting horizons Vector Autoregressive models performed at least as well as structural econometric models. It was only over longer term forecasting horizons that the structural models which incorporated economic theory were unambiguously superior.

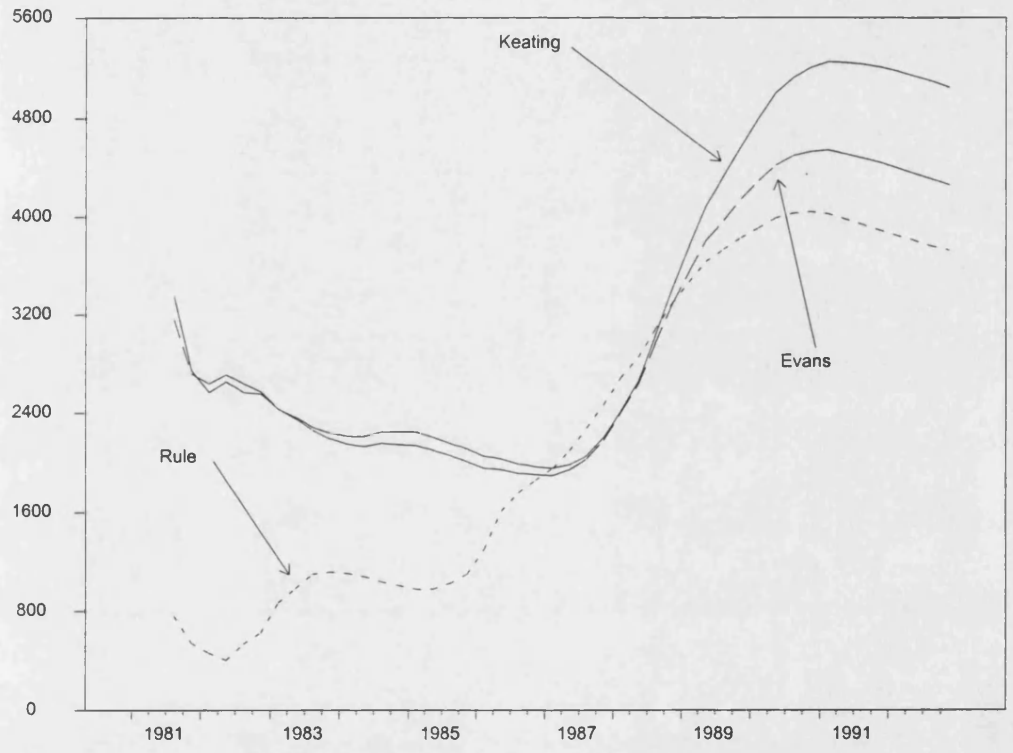
Figure 7: Forecast Errors For equations based on theory and data fitting



However, in the case of the consumption functions shown in Figure 7 this does not appear to be the case. In fact, from Figure 7 it looks as if the atheoretical data fitting model performs the best throughout the entire forecasting period. This intuition can be tested more formally, and a common way is to calculate the Root Mean Square Error[47] of the different functions throughout forecast period. The RMSE for each of the three consumption functions are shown Figure 8.

From Figure 8 it is apparent that, with the exception of the middle portion of the graph (i.e. approximately the period from 1987 to 1988) the consumption functions based on economic theory forecast less well than the atheoretical version. This difference is particularly marked for the first five years or so of the forecast period. Thereafter the RMSE increases quite dramatically for all three functions, but from about 1989 onwards the forecasts produced by the data fitting method are superior. The only conclusion which can be drawn from Figure 8 is that the theory introduced by Keating and defended with econometrics has added little, if anything to the forecasting abilities of the model.

Figure 8: Root Mean Square Errors for Equations based on Theory and Data Fitting



Indeed it seems that using economic theory to inform the specification of the consumption function has actually produced a worse result, and it is this aspect which is the most troubling. It seems from Figure 8 that, for the 2 to 3 year time frame that most forecasters are really interested in, economic theory is detrimental to the forecasting ability of the model.

Of course, as noted, it is possible to counter these sort of criticisms by referring to other reasons for wanting economic theory in one's model; for example, tractable long run properties, developing a theoretical understanding of the economy and so on.

However, a sceptic would surely maintain that if these theories are neither supported by the data nor useful for forecasting then their relevance to practical matters must surely be in doubt.

Conclusion

In this chapter I have looked at the estimation of a single equation and shown how econometrics and judgement are interwoven in the specification of an equation. From the very beginning of the analysis, when a demand-based specification was chosen and supply-side variables omitted, judgement played an important role. When statistically insignificant variables were included and other, apparently equally insignificant ones excluded, it is the economist who makes this judgement. When testing restrictions, the crucial decision is deciding what level of statistical significance is appropriate. Thus, whether or not the hypothesis should be rejected is likewise a matter of judgement and again it is the economist who decides.

But note this was only one equation, using just four variables. A typical macroeconomic modeller may use up to five hundred variables and estimate several dozen equations, each of which will need the same combination of econometrics, economics and conviction as the consumption function detailed above. What this chapter has shown then is that the foundations of macro-econometric modelling are set not just in the (relative) certainty of econometric procedures and statistical theory, but also in the social practices and shared convictions of economists. However, this foundational role is not the only way in which the social enters into macro-econometrics; it is merely the beginning.

Estimating equations and combining them into a coherent model is but a prelude to the real business of macro-econometric modelling - which is either policy analysis or making money depending on your employer (although the two are connected). It is hoped that the rigor of economic theory and the empirical analysis of econometrics will combine to identify behavioural laws, or at least regularities, which can then be run forward in time to show what the future will bring. Unfortunately economic modelling and forecasting does not appear work like this. Instead, as this chapter has shown, empirical analysis in econometrics, whether used by experts like Keating or novices such as myself, can fall some way short of providing convincing evidence for economic theories.

Notes

[1] The full reference is: Keynes, J.M. (1936) *The General Theory of Employment, Interest and Money.* Harcourt Brace, New York pp. 161-3

[2] If the aim is simply to test the theory then the stability of the co-efficients is less important than their statistical significance.

[3] Winch, P. (1958/1988) The Idea of a Social Science and its Relationship to Philosophy. London and New York: Routledge.

[4] See, e.g. Keynes op cit. note 1 or Keynes, J.M. (1939) 'Professor Tinbergen's Method', *Economic Journal*, Vol. 49, pp. 558-68.

[5] Keating, G. (1985) *The Production and Use of Economic Forecasts.* London: Methuen. According to his entry in *Who's Who in Economics*, Keating studied economics at Oxford and the London School of Economics and was a Research Fellow at the London Business School. His areas of expertise are macroeconometric modelling and forecasting, and domestic and international financial systems. He is currently Chief Economist and Director of Research for Credit Suisse First Boston Ltd.

[6] Diagram adapted from Begg, D., Fisher, S. And Dornbusch, R. (1991) *Economics: 3rd Edition.* London: McGraw-Hill, p. 361.

[7] Diagram adapted from Begg, D., Fisher, S. And Dornbusch, R. (1991) *Economics: 3rd Edition.* London: McGraw-Hill, p. 361.

[8] Pindyck, R.S. and Rubinfeld, D.L. (1991) *Econometric Models and Economic Models: 3rd edition.* New York: McGraw-Hill International Editions.

[9] See e.g. the recent review by the ESRC Macroeconomic Modelling Bureau: Church, K.B., Mitchell, P.R., Smith, P.N. and Wallis, K.F. (1993) 'Comparative Properties of Models of the UK Economy'. *National Institute Economic Review*, August 1993, pp. 87-100, esp. Box A, p. 89.

[10] op cit. note 5.

[11] This is the Experimenters' Regress. See Collins, H.M. (1985) *Changing Order: Replication and Induction in Scientific Practice.* Sage: London.

[12] Evans, R. (1993) 'Macro-econometric Modelling: A Sociological Perspective'. Unpublished M.Sc. dissertation, University of Bath.

[13] Kuhn, T.S. (1961) 'The function of measurement in modern physical science'. *Isis*, **52**, 162-76. Reprinted as 'Normal Measurement and Reasonable Agreement', in Barnes, B. And Edge, D. (Eds.) *Science in Context.* Milton Keynes: Open University Press.

[14] In fact, this was the context in which I first came across Keating's book which was recommended to me by Andrew Britton, director of the National Institute for Economic and Social Research.

[15] Keating is remarkable for the detailed references he gives. This is actually very unusual amongst economists and replication of econometric work is usually founders because would-be replicators are unable to re-construct the dataset. However this is not the only problem. In a 1986 paper, Dewald, Thursby and Anderson found that datasets and programs submitted with journal articles 'were often so inadequately documented that [they] could not identify the variables which had been used in calculating the published empirical results' (p. 592). In many case, Dewald *et al* found that replication was only possible after extensive consultation with, and active assistance from, the economist who had submitted the paper. In some cases even this was not enough to reproduce the results. Dewald, W.G., Thursby, J.G. and Anderson, R.G. (1986) 'Replication in Empirical Economics: The *Journal of Money, Credit and Banking* Project.' *American Economic Review*, **76**, No. 4, pp. 587-603.

[16] The data series used were as follows, the four letter code in parentheses indicates the CSO codes for that series: C = Consumers' Expenditure in 1990 prices (CAAB); PC = Consumer Price Index, calculated by dividing Consumers' Expenditure in current prices (AIIX) by Consumers' Expenditure in 1990 prices (CAAB) and multiplying the result by 100; RPDI = Real Personal Disposable Income, calculated by dividing Personal Disposable Income in current prices (AIIW) by (PC/100); RLB = Average Discount Rate on 91 Day Bill (AJNB). In Keating's model, RLB is the UK banks' base rates. However, I was unable to find a sufficiently long run of figures for this and was forced to used the average discount rate instead.

[17] The full details of the regression output are reproduced below. Note that although the sample period was set at 1955Q1 to 1992Q4, one of the data series does not extend that far back and the estimation only includes data from 1964 onwards. In the regression program:

```
LOGC = LOG(C)
RPDI = YD/(PC/100)
LOGRPDI = LOG(RPDI)
PCRATIO = LOG((PC/PC{1}) + 0.0057)

Dependent Variable LOGC - Estimation by Least Squares
Quarterly Data From 1964:01 To 1992:04
Usable Observations     116      Degrees of Freedom    103
Centered R**2      0.997059      R Bar **2    0.996717
Uncentered R**2    0.999999      T x R**2      116.000
Mean of Dependent Variable      11.013402234
Std Error of Dependent Variable  0.212329342
Standard Error of Estimate       0.012166722
Sum of Squared Residuals        0.0152469988
Regression F(12,103)               2910.1168
Significance Level of F           0.00000000
Durbin-Watson Statistic              1.966016
Q(29)                               27.129075
Significance Level of Q            0.56474111

   Variable                    Coeff        Std Error      T-Stat     Signif
*******************************************************************************
1.  Constant                -0.124365445  0.110400318     -1.12650  0.26257361
2.  LOGC{1}                  0.724621821  0.106667865      6.79325  0.00000000
3.  LOGC{2}                  0.415723533  0.125809593      3.30439  0.00131007
4.  LOGC{4}                 -0.194442601  0.095145123     -2.04364  0.04354130
5.  LOGRPDI{1}               0.213586757  0.081052181      2.63518  0.00970793
6.  LOGRPDI{2}              -0.214166163  0.089530596     -2.39210  0.01856531
7.  LOGRPDI{4}               0.067253889  0.078345645      0.85843  0.39265086
8.  RLB{1}                  -0.001567148  0.001011728     -1.54898  0.12445331
9.  RLB{2}                  -0.000540274  0.001186537     -0.45534  0.64982551
10. RLB{4}                   0.000536805  0.000813624      0.65977  0.51087352
11. PCRATIO{1}              -0.077706992  0.154128904     -0.50417  0.61521950
12. PCRATIO{2}               0.049429281  0.154739128      0.31944  0.75004273
13. PCRATIO{4}              -0.022577034  0.127275948     -0.17739  0.85955318
```

[18] In his Ph.D. Bernard Walters examines the sensitivity of Layard and Nickel's labour market econometrics to the vintage of the data set and finds that an initially supported hypothesis is rejected by later versions of the data.

[19] Subject to the caveat that data revisions have not changed the economic history to such an extent that completely new relationships throughout the entire period are now implied

[20] Full regression output is shown below, all variables as defined in note 17

```
Dependent Variable LOGC - Estimation by Least Squares
Quarterly Data From 1965:04 To 1980:04
Usable Observations      57      Degrees of Freedom     44
 Total Observations      61      Skipped/Missing         4
Centered R**2      0.980499      R Bar **2    0.975181
Uncentered R**2    0.999999      T x R**2       57.000
Mean of Dependent Variable      10.897451160
Std Error of Dependent Variable  0.095194570
Standard Error of Estimate       0.014997032
Sum of Squared Residuals        0.0098960827
Regression F(12,44)                184.3605
Significance Level of F           0.00000000
Durbin-Watson Statistic              2.108635
Q(15)                               10.929761
Significance Level of Q            0.75755739

   Variable                    Coeff        Std Error      T-Stat     Signif
*******************************************************************************
1.  Constant                -0.715608347  0.496902667     -1.44014  0.15690627
2.  LOGC{1}                  0.364769308  0.193977968      1.88047  0.06667384
3.  LOGC{2}                  0.391825177  0.195417640      2.00507  0.05113193
4.  LOGC{4}                  0.071760777  0.181901830      0.39450  0.69511563
5.  LOGRPDI{1}               0.281349618  0.146594839      1.91923  0.06145503
6.  LOGRPDI{2}              -0.080405825  0.162207397     -0.49570  0.62257466
7.  LOGRPDI{4}               0.039305984  0.134872185      0.29143  0.77209096
8.  RLB{1}                  -0.002383106  0.001897493     -1.25592  0.21577203
9.  RLB{2}                  -0.002058504  0.002030083     -1.01400  0.31612658
10. RLB{4}                   0.000026294  0.001595276      0.01648  0.98692399
11. PCRATIO{1}              -0.216661697  0.261471885     -0.82862  0.41179004
12. PCRATIO{2}               0.066755344  0.280871316      0.23767  0.81323851
13. PCRATIO{4}              -0.152298981  0.212750691     -0.71586  0.47786197
```

[21] See, e.g. Wren-Lewis, S. (1992) Macroeconomic Theory and UK Macroeconomic Models: Another Failed Partnership?'. International Centre for Macroeconomcic Modelling, University of Strathclyde. Discussion paper number 9.

[22] op cit. note 11

[23] The classic case of the Experimenter's Regress probably concerns the existence or otherwise of high fluxes of gravity waves and is described in detail in H.M. Collins (1985) *Changing Order: Replication and Induction in Scientific Practice.* London: Sage.

[24] For more on this particular debate see: Friedman, M. And Schwartz, A.J. (1982) *Monetary Trends in the United States and the United Kingdom: Their Relation to Income, Prices and Interest Rates, 1867-1975* Chicago: University of Chicago Press; Hendry, D.F. and Ericsson, N.R. (1987) 'Assertion Without Empirical Basis: An Econometric Appraisal of *Monetary Trends ..... In The United Kingdom* by Milton Friedman and Anna J. Schwartz'. University of Oxford, Applied Economics Discussion Paper No. 25, March 1987.

[25] Full regression output is shown below, all variables as defined in note 17:

```
Dependent Variable LOGC - Estimation by Least Squares
Quarterly Data From 1963:03 To 1992:04
Usable Observations     118      Degrees of Freedom    109
Centered R**2      0.996995      R Bar **2    0.996774
Uncentered R**2    0.999999      T x R**2      118.000
Mean of Dependent Variable      11.007661818
Std Error of Dependent Variable  0.215036430
Standard Error of Estimate       0.012213594
Sum of Squared Residuals        0.0162597360
Regression F(8,109)                4519.8681
Significance Level of F           0.00000000
Durbin-Watson Statistic             1.943379
Q(29)                              33.951961
Significance Level of Q           0.24102305

   Variable                  Coeff        Std Error      T-Stat     Signif
*******************************************************************************
1.  Constant              -0.151381017  0.093847795     -1.61305  0.10962560
2.  LOGC{1}                0.725039309  0.104477986      6.93964  0.00000000
3.  LOGC{2}                0.264417707  0.104975072      2.51886  0.01322480
4.  LOGRPDI{1}             0.232492799  0.077070393      3.01663  0.00318148
5.  LOGRPDI{2}            -0.206278514  0.079660199     -2.58948  0.01092416
6.  RLB{1}                -0.001348282  0.000970975     -1.38859  0.16778987
7.  RLB{2}                -0.000455812  0.001020377     -0.44671  0.65597137
8.  PCRATIO{1}            -0.030471083  0.143911740     -0.21173  0.83270955
9.  PCRATIO{2}            -0.001311091  0.134775165     -0.00973  0.99225609
```

[26] The same is also true of the impact of interest rates on investment where economic theory expects a clear connection which econometrics has yet to find; see Evans, R. (1993) 'Soothsaying or Science: falsification, Uncertainty and Social Change in Macroeconometric Modelling'. Unpublished M.Sc. dissertation, University of Bath.

[27] Full regression output is shown below, all variables as defined in note 17. In addition:

```
CDIFF = LOGC - LOGC{1}


Dependent Variable LOGC - Estimation by Least Squares
Quarterly Data From 1963:03 To 1992:04
Usable Observations     118      Degrees of Freedom    110
Centered R**2      0.996995      R Bar **2    0.996803
Uncentered R**2    0.999999      T x R**2      118.000
Mean of Dependent Variable      11.007661818
Std Error of Dependent Variable  0.215036430
Standard Error of Estimate       0.012157957
Sum of Squared Residuals        0.0162597502
Regression F(7,110)                5212.9495
Significance Level of F           0.00000000
Durbin-Watson Statistic             1.943329
Q(29)                              33.932988
Significance Level of Q           0.24172173

   Variable                  Coeff        Std Error      T-Stat     Signif
*******************************************************************************
1.  Constant              -0.151510755  0.092472159     -1.63845  0.10418572
2.  LOGC{1}                0.724865368  0.102467648      7.07409  0.00000000
3.  LOGC{2}                0.264678161  0.101040980      2.61951  0.01004840
4.  LOGRPDI{1}             0.232378425  0.075821339      3.06482  0.00274011
5.  LOGRPDI{2}            -0.206237816  0.079187882     -2.60441  0.01047460
```

```
6.  RLB{1}                  -0.001347448  0.000962777     -1.39954  0.16446403
7.  RLB{2}                  -0.000457985  0.000991076     -0.46211  0.64491566
8.  PCRATIO{1}              -0.031367560  0.110032252     -0.28508  0.77612193

Dependent Variable CDIFF - Estimation by Least Squares
Quarterly Data From 1963:03 To 1992:04
Usable Observations    118      Degrees of Freedom   110
Centered R**2     0.196310      R Bar **2   0.145166
Uncentered R**2   0.331732      T x R**2      39.144
Mean of Dependent Variable      0.0058944263
Std Error of Dependent Variable 0.0131498134
Standard Error of Estimate      0.0121579567
Sum of Squared Residuals        0.0162597502
Regression F(7,110)                   3.8384
Significance Level of F         0.00090327
Durbin-Watson Statistic           1.943329
Q(29)                            33.932988
Significance Level of Q          0.24172173

   Variable                     Coeff       Std Error      T-Stat     Signif
********************************************************************************
1.  Constant                -0.151510755  0.092472159     -1.63845  0.10418572
2.  LOGC{1}                 -0.275134633  0.102467648     -2.68509  0.00837366
3.  LOGC{2}                  0.264678162  0.101040980      2.61951  0.01004840
4.  LOGRPDI{1}               0.232378425  0.075821339      3.06482  0.00274011
5.  LOGRPDI{2}              -0.206237816  0.079187882     -2.60441  0.01047460
6.  RLB{1}                  -0.001347448  0.000962777     -1.39954  0.16446403
7.  RLB{2}                  -0.000457985  0.000991076     -0.46211  0.64491566
8.  PCRATIO{1}              -0.031367560  0.110032252     -0.28508  0.77612192
```

[28] Full regression details are shown below, all variables as defined in note 27. In addition:

```
RPDIDIFF = LOGRPDI - LOGRPDI{1}
CRDPI = LOGC - LOGRPDI

Dependent Variable CDIFF - Estimation by Least Squares
Quarterly Data From 1963:03 To 1992:04
Usable Observations    118      Degrees of Freedom   110
Centered R**2     0.196310      R Bar **2   0.145166
Uncentered R**2   0.331732      T x R**2      39.144
Mean of Dependent Variable      0.0058944263
Std Error of Dependent Variable 0.0131498134
Standard Error of Estimate      0.0121579567
Sum of Squared Residuals        0.0162597502
Regression F(7,110)                   3.8384
Significance Level of F         0.00090327
Durbin-Watson Statistic           1.943329
Q(29)                            33.932988
Significance Level of Q          0.24172173

   Variable                     Coeff       Std Error      T-Stat     Signif
********************************************************************************
1.  Constant                -0.151510755  0.092472159     -1.63845  0.10418572
2.  LOGC{1}                  0.015684138  0.008553013      1.83376  0.06939373
3.  CDIFF{1}                -0.264678162  0.101040980     -2.61951  0.01004840
4.  RPDIDIFF{1}              0.206237816  0.079187882      2.60441  0.01047460
5.  CRDPI{1}                -0.026140609  0.067234148     -0.38880  0.69817596
6.  RLB{1}                  -0.001347448  0.000962777     -1.39954  0.16446403
7.  RLB{2}                  -0.000457985  0.000991076     -0.46211  0.64491566
8.  PCRATIO{1}              -0.031367560  0.110032252     -0.28508  0.77612192
```

[29] Full regression details are shown below, all variables as defined in note 27:

```
LINREG CDIFF
# CONSTANT CDIFF{1} RPDIDIFF{1} CRDPI{1} RLB{1 2} PCRATIO{1}

Dependent Variable CDIFF - Estimation by Least Squares
Quarterly Data From 1963:03 To 1992:04
Usable Observations    118      Degrees of Freedom   111
Centered R**2     0.171741      R Bar **2   0.126970
Uncentered R**2   0.311303      T x R**2      36.734
Mean of Dependent Variable      0.0058944263
Std Error of Dependent Variable 0.0131498134
Standard Error of Estimate      0.0122866678
Sum of Squared Residuals        0.0167568049
Regression F(6,111)                   3.8360
Significance Level of F         0.00163075
Durbin-Watson Statistic           1.970409
Q(29)                            31.440985
```

```
Significance Level of Q           0.34493724

   Variable                      Coeff        Std Error      T-Stat     Signif
*******************************************************************************
1.  Constant                  0.017700505  0.006087038      2.90790  0.00439517
2.  CDIFF{1}                 -0.271099684  0.102049314     -2.65656  0.00905764
3.  RPDIDIFF{1}               0.242975067  0.077422832      3.13829  0.00217692
4.  CRDPI{1}                  0.020672472  0.062857552      0.32888  0.74286751
5.  RLB{1}                   -0.000940768  0.000946805     -0.99362  0.32256724
6.  RLB{2}                    0.000146405  0.000944561      0.15500  0.87710475
7.  PCRATIO{1}               -0.090857744  0.106254092     -0.85510  0.39433837
```

[30] It is of course a widely held suspicion that most hypotheses can be supported by a regression analysis if one is prepared to keep trying.

[31] Full regression details are shown below, all variables as defined in note 27. In addition:

```
NEWCDIFF = LOGC - LOGC{1} - 0.0057
NEWRPDI = LOGRPDI - LOGRPDI{1} - 0.0057

Dependent Variable CDIFF - Estimation by Least Squares
Quarterly Data From 1963:03 To 1992:04
Usable Observations    118      Degrees of Freedom   111
Centered R**2      0.171740      R Bar **2    0.126969
Uncentered R**2    0.311303      T x R**2       36.734
Mean of Dependent Variable      0.0058944263
Std Error of Dependent Variable 0.0131498134
Standard Error of Estimate      0.0122866741
Sum of Squared Residuals        0.0167568221
Regression F(6,111)                   3.8360
Significance Level of F         0.00163083
Durbin-Watson Statistic           1.970410
Q(29)                            31.441066
Significance Level of Q         0.34493355

   Variable                      Coeff        Std Error      T-Stat     Signif
*******************************************************************************
1.  CDIFF{1}                 -0.271098125  0.102049322     -2.65654  0.00905803
2.  RPDIDIFF{1}               0.242976266  0.077422918      3.13830  0.00217684
3.  CRPDI                     0.020673571  0.062857607      0.32890  0.74285455
4.  RLB{1}                   -0.000940766  0.000946806     -0.99362  0.32256901
5.  RLB{2}                    0.000146400  0.000944562      0.15499  0.87710898
6.  PCRATIO{1}               -0.090350552  0.105669229     -0.85503  0.39437528
7.  Constant                  0.017699217  0.006086986      2.90771  0.00439761
```

[32] Source: Gavyn Davies, Panel of Independent Forecasters Report, July 1993, H.M. Treasury. P. 49.

[33] The retrenchment which has taken place over the last few years, as evidenced by the exceptionally high savings ratio, suggests that consumers are now taking steps to restore their balance sheets. Thus it is possible that in a few years the data might, once again, support the hypothesis.

[34] Full regression details are shown below, all variables as defined in note 27. In addition:

```
RLBDIFF = RLB - RLB{1}

Dependent Variable NEWCDIFF - Estimation by Least Squares
Quarterly Data From 1963:03 To 1992:04
Usable Observations    118      Degrees of Freedom   112
Centered R**2      0.141944      R Bar **2    0.103638
Uncentered R**2    0.142133      T x R**2       16.772
Mean of Dependent Variable      0.0001944263
Std Error of Dependent Variable 0.0131498134
Standard Error of Estimate      0.0124497704
Sum of Squared Residuals        0.0173596397
Durbin-Watson Statistic            1.965772
Q(29)                             38.290652
Significance Level of Q          0.11600787

   Variable                      Coeff        Std Error      T-Stat     Signif
*******************************************************************************
1.  NEWCDIFF{1}              -0.191405800  0.095181082     -2.01096  0.04673122
2.  NEWRPDI{1}                0.210140959  0.076663471      2.74108  0.00712958
3.  CRPDI                    -0.072370013  0.042787261     -1.69139  0.09354281
4.  RLB{1}                   -0.000585757  0.000424599     -1.37955  0.17047260
5.  RLBDIFF{1}               -0.000217112  0.000956428     -0.22700  0.82083493
6.  PCRATIO{1}               -0.090701326  0.107071760     -0.84711  0.39874152
```

[35] Hendry, D.F. (1989) PC-Give: An Interactive Econometric Modelling System, version 6.0/6.01.

[36] Note that this is not necessarily a bad thing as all theories take time to establish and thus a certain tenacity may be an admirable quality in a scientist

[37] Adapted from Hudson, J. and Dymiotou-Jensen, M. (1989) *Modelling a Developing Country: A Case Study of Cyprus*. England: Avebury. p. 55.

[38] Paraphrase of Prof. Assar Lindbeck, quoted in Parkin, M. and King, D. (1992) *Economics*. England: Addison-Wsley Publishing. p. 3.

[39] For details of the equations used see Table 1 and Table 2. See also notes 17 and 20.

[40] For example, all the co-efficients on lagged consumers expenditure are positive, implying that consumers expenditure can only increase. Professor Christopher Heady has also pointed out that the coefficients on these variables sum to one, which is an undesirable outcome.

[41] This question was addressed directly by the National Institute in a report which analyses forecasting performance in the UK during the 1980s. Britton and Pain conclude that:

> There only appears to be evidence of structural breaks in the relationships for investment and disposable income when the latest outturns are used, although the statistics for GDP, domestic demand and inflation are significant at the 10% level ... [However] if the null [i.e. that there is no structural break] is rejected for GDP, inflation, employment and disposable income

Britton, A. And Pain, N. (1992) Economic Forecasting in Britain. National Institute of Economic and Social Research, Report Series, Number 4.

[42] Full details of the regression output are shown below, all variables as defined in note 27. The comparable figure to the one quoted by Keating is 0.0124994245, which seems remarkably similar with Keating's figure.

```
Dependent Variable NEWCDIFF - Estimation by Least Squares
Quarterly Data From 1963:03 To 1992:04
Usable Observations     118      Degrees of Freedom    113
Centered R**2      0.127363      R Bar **2    0.096474
Uncentered R**2    0.127556      T x R**2       15.052
Mean of Dependent Variable      0.0001944263
Std Error of Dependent Variable 0.0131498134
Standard Error of Estimate      0.0124994245
Sum of Squared Residuals        0.0176546242
Durbin-Watson Statistic            1.990171
Q(29)                             38.572758
Significance Level of Q            0.11011943

   Variable                      Coeff        Std Error      T-Stat     Signif
*******************************************************************************
1.  NEWCDIFF{1}              -0.210385218  0.094557184     -2.22495  0.02807056
2.  NEWRPDI{1}                0.235498640  0.074724145      3.15157  0.00207932
3.  CRPDI                    -0.027616909  0.028010672     -0.98594  0.32626776
4.  RLBDIFF{1}               -0.000508763  0.000936491     -0.54327  0.58801681
5.  PCRATIO{1}               -0.123587235  0.104800741     -1.17926  0.24077192
```

[43] op cit. note 5, p. 83.

[44] Full details of the regression equation are given below, all variables as defined in note 27

```
Dependent Variable LOGC - Estimation by Least Squares
Quarterly Data From 1956:04 To 1980:04
Usable Observations     71       Degrees of Freedom     68
 Total Observations     97       Skipped/Missing        26
Centered R**2      0.985944      R Bar **2    0.985531
Uncentered R**2    0.999998      T x R**2       71.000
Mean of Dependent Variable      10.858237641
Std Error of Dependent Variable  0.116942481
Standard Error of Estimate       0.014066732
Sum of Squared Residuals        0.0134553606
Durbin-Watson Statistic            2.259186
Q(24)                             21.207475
Significance Level of Q            0.62645734

   Variable                      Coeff        Std Error      T-Stat     Signif
*******************************************************************************
1.  LOGC{1}                   0.698102447  0.107602802      6.48777  0.00000001
2.  LOGRPDI{1}                0.301617075  0.106933656      2.82060  0.00627647
3.  RLB{1}                   -0.002760012  0.000702115     -3.93099  0.00020073
```

[45] See e.g. Evans, R., op cit. note 26

[46] Wallis, K. F., & Whitely, J. (1991). 'Sources of Error in Forecasts and Expectations: UK Economic Models, 1984-8'. *Journal of Forecasting*, Vol. 10, pp. 231-253.

[47] Formulae for calculating RMSE are as follows, where X_f = forecast and X_t = actual:

$$\text{Mean Square Error (MSE)} = \Sigma(X_f - X_t)^2 / (t - 2); \text{RMSE} = \sqrt{\text{MSE}}$$

Source: Ramanathan, R. (1992) *Introductory Econometrics; with applications. 2nd Edition.* Florida, USA: Harcourt Brace Jovanovich International. p. 115.

Chapter 3

The previous chapter examined in detail the specification and estimation of a single econometric equation. The conclusion drawn was that the foundations of an econometric model are sunk not just in the observed regularities of economic data, but also in the shared theoretical convictions of economists. In this chapter I will extend this argument to include not just the specification of econometric equations but also their use in econometric forecasting. The chapter thus makes one major claim: economic forecasts are not extrapolations of the past into the future, but considered judgements about what is most likely to happen. The argument thus concerns the nature of expertise and skill. It is not a denunciation of macro-econometric modelling, but a re-evaluation of the basis of its claims to knowledge. If economic forecasts are products of considered judgement and not the inevitable consequence of the application of immutable laws and forces then attitudes to economic forecasting, especially as it impinges on the making of economic policy, will need to be re-examined. However, this is the task of later chapters. In this chapter I merely want to describe how an economic forecast is produced.

The purpose of the exposition is not to argue that macro-econometric modelling is of no value whatsoever, and the argument is not necessarily a negative one. Economic modellers and forecasters probably know more about the economy than any one else, and they possess this knowledge because they have spent most of their working lives trying to model it. Constructing, maintaining and updating macro-econometric models is a full-time job which compels a small group of people to continuously monitor the economy and by this process to become experts about it.

Instead, the aim of this chapter is to illustrate these points and demonstrate, in fairly general terms, the importance of judgement in the production of economic forecasts. A more detailed analysis of the way in which specific judgements affect forecasts is given in the following chapters. The aim now is to substantiate the claim that judgement matters and to introduce the practical experience of forecasting. The chapter does this by showing how a macro-econometric model is used to produce an economic forecast. Drawing on the first meeting of the Panel of Independent Forecasters, I demonstrate that it is not just the model which determines the forecast, but the also the judgement of the economist(s). The argument is illustrated by showing how

a single econometric model can be used to produce forecasts consistent with any of the Seven Wise Men's individual positions.

The Model to Model all Models?

In this section I outline my version of the model estimated by Keating. The theoretical basis of the model was outlined in Chapter 2, here I present a more practical representation of the model. By using a flow chart one can identify the variables which make up the model and represent diagrammatically how they are inter-related. A flow chart for my model is shown in Figure 1, overleaf.

In the chart, all variables enclosed in ellipses are endogenous, which means that their future values are forecast by the model. Variables enclosed in rectangular boxes are exogenous which means that they are not forecast by the model and that any future values must be forecast by some other method. One way is to input the values directly (this is done for the Expected Exchange Rate and Interest Rate) Another method is to use a simple univariate autoregression to extrapolate forwards from the base values (this is used for the majority of exogenous variables). A more complex alternative, not used in my model, would be to use a multi-variate autoregression model to produce the exogenous projections.

Lines joining the boxes show the links within the economy with arrows indicating 'causality'. Where a line splits or branches to influence two or more variables this is represented by a small triangle at the junction. If no triangular symbol is present, this means that the lines simply cross and do not interact with each other in anyway (the right-angled nature of this crossing attempts to imply the orthogonal nature of the two forces). This is done to assist in distinguishing between the T-junction where a path branches and the crossroads where two lines simply cross.

In addition to elliptical and rectangular boxes, there are also three lozenge shaped rectangular boxes with rounded corners (Interest Rates, in the upper left quadrant of the chart, and the Balance of Payments and the Public Sector Borrowing Requirement at the bottom). These boxes are used to identify variables which, in Keating's original specification of the model these variables were endogenous, but which in my model are exogenous.

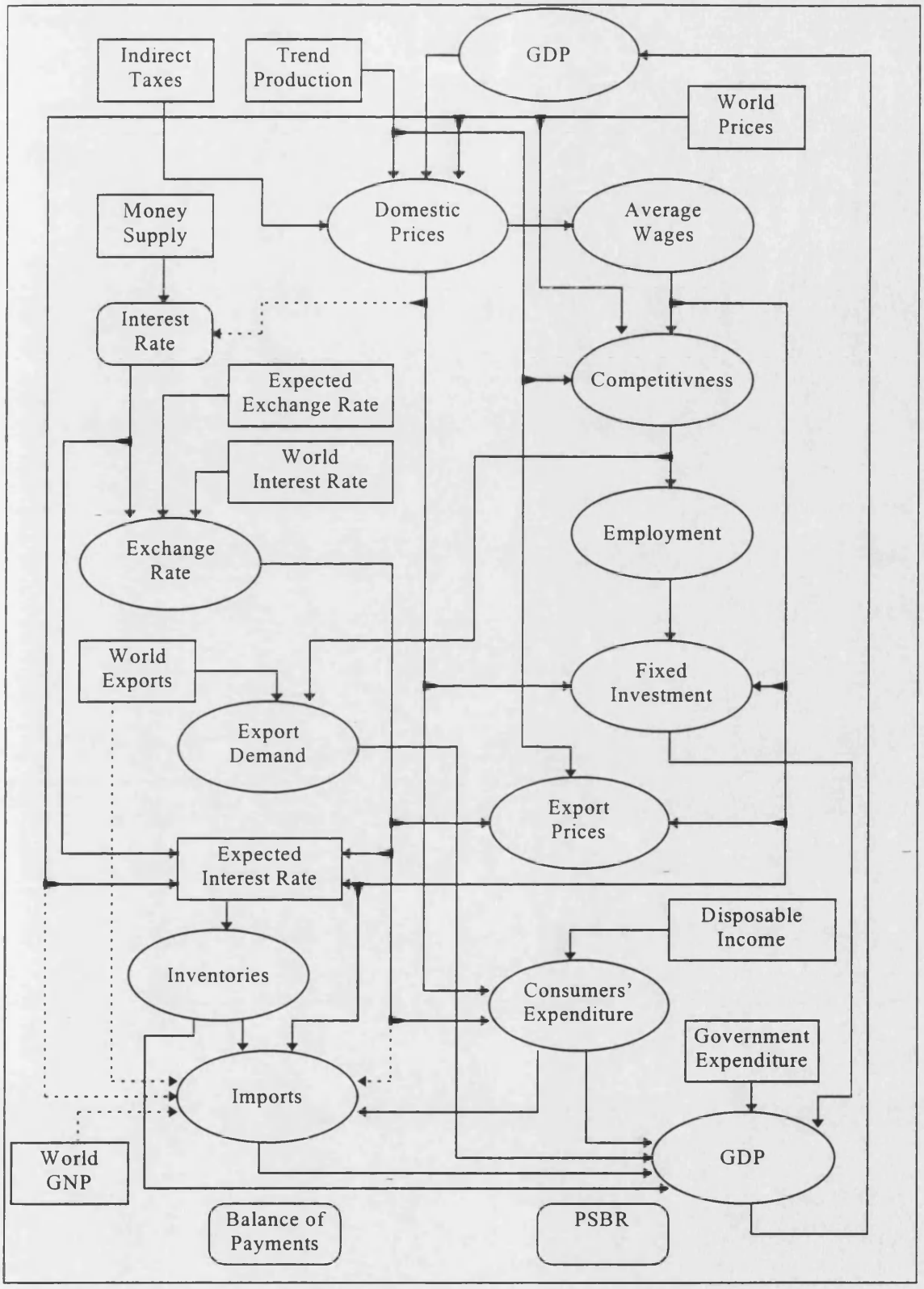


Figure 1: Flow Chart of Macro-Econometric Model

In the case of Interest Rates this decision was made in order to follow the format adopted by the Treasury's Panel of Forecasters in their reports. The dotted line linking prices to interest rates shows the channel of influence which has been cut as a result of this decision. The PSBR and Balance of Payments were made exogenous for rather

more pragmatic reasons: the equations given by Keating simply did not fit my data at all well (possibly due to changes in tax structure and rates etc. during 1980s) and forecasting these variables using autoregressive equations proved far more successful than attempting to reproduce the specifications used in Keating's model.

Other changes from the model specified by Keating concern the variables which influence Imports. In Keating's model, Imports are influenced by several 'world variables' (e.g. World GNP, Prices and the Exchange Rate). These variables are not included in the specification of my Import equation as I was unable to obtain an appropriate data series. These variables thus had to be dropped from the equation. Once again the dotted lines reflect the channels of influence which one might ordinarily expect to find in a macro-econometric model. I now turn to a discussion of the model of the economy represented in Figure 1.

In the model shown in Figure 1, the single most important variable is Domestic Prices, which are determined by the level of GDP from the previous iteration or time period and several exogenous variables (Indirect taxes, Trend Production and World Prices). Once forecast, Prices directly influence Average Wages, Fixed Investment and Consumers' Expenditure. Through their effect on Wages, Prices also influence Competitiveness, Export Prices, the Expected Interest Rate, Inventories and Imports. In addition, Competitiveness, which is responsive to Prices because of their effect on Wages, also influences Export Demand.

In Keating's model, the influence of Prices is even greater. Remember that the Interest Rate should be endogenous and is, in part, determined by Prices. If this linkage had been maintained, there would be an additional set of effects. In particular the Exchange Rate would be responsive to changes in Prices and these effects would, in turn, impact upon the Expected Rate of Interest (and hence Inventories) as well as Imports.

Figure 1 thus shows the basic structure of the econometric model, and by following the connections between variables it is possible to see how a change in one variable can propagate through the system. Thus, for example, an increase in Prices might push up Wages, thereby reducing Competitiveness and thus Employment and Fixed Investment. The increase in Wages might be expected to raise Export Prices and also to increase the Expected Rate of Interest (which would have to increase to maintain the real rate of interest at a particular level). In addition to increasing the Expected Rate

of Interest, a rise in Prices would also increase the actual current or nominal Rate of Interest and hence the Exchange Rate (because sterling deposits become increasingly attractive due to their higher rate of return). This would happen directly in Keating's model, and indirectly in my own as the implications of the change in Prices was taken into account when setting the exogenous variables. Finally, the rise in Prices would also have impacts upon Consumers' Expenditure decisions (for example by encouraging precautionary savings).

Clearly, the structure given in Figure 1 can be used to develop rich and complex stories about economic events. The problem is that although we can say that the increase in Prices will push up Wages, reduce Competitiveness and hence Employment, we cannot say by how much, nor how soon. For governments and businesses alike these are the really important questions. A government planning to raise additional revenue through increasing Indirect Taxes (one of the variables which directly influences Prices) would obviously like to know what the consequences of choosing a particular tax-raising policy would be. Similarly, a firm planning its investment or marketing strategy would like to think it had some idea about the likely level of Consumers' Expenditure and the Rate of Interest applied to any loans it may have to repay. Unfortunately it is just this kind of detail that the flow chart cannot provide. Although it may be a convenient way of organising one's thoughts it is not much use for the sort of decisions that people who rely on economic forecasts are called upon to make. These people need numbers, and this is what an econometric model provides.

An econometric model is simply a mathematical version of the flow chart shown in Figure 1, but the lines linking variables have been replaced with numerical weights. In addition, a time dimension can also be added by allowing (several) previous values of a particular variable to appear in each equation. Thus the effects of Prices on Wages and ultimately Employment might be seen very quickly or relatively slowly. The mathematical version of the model is listed in Appendix A and Appendix B gives details of the data series used. In the remainder of this chapter I illustrate how an economic forecast is produced. In this case the forecast is the average of the Seven Wise Men's, as detailed in their report of February 1993.

The Seven Wise Men and Economic Forecasting

In this section I will use the Treasury's Panel of Independent Forecasters to show that even if it were possible for economic modellers to agree on one unique model of the economy (in this case the one used by Keating and myself) they could still disagree about the future prospects. The use of different models certainly reflects differences of both opinion and economics between the Wise Men. However, it would be a mistake to think that the models were the cause of these differences and that, if only they could agree on a model, they would agree on everything else.

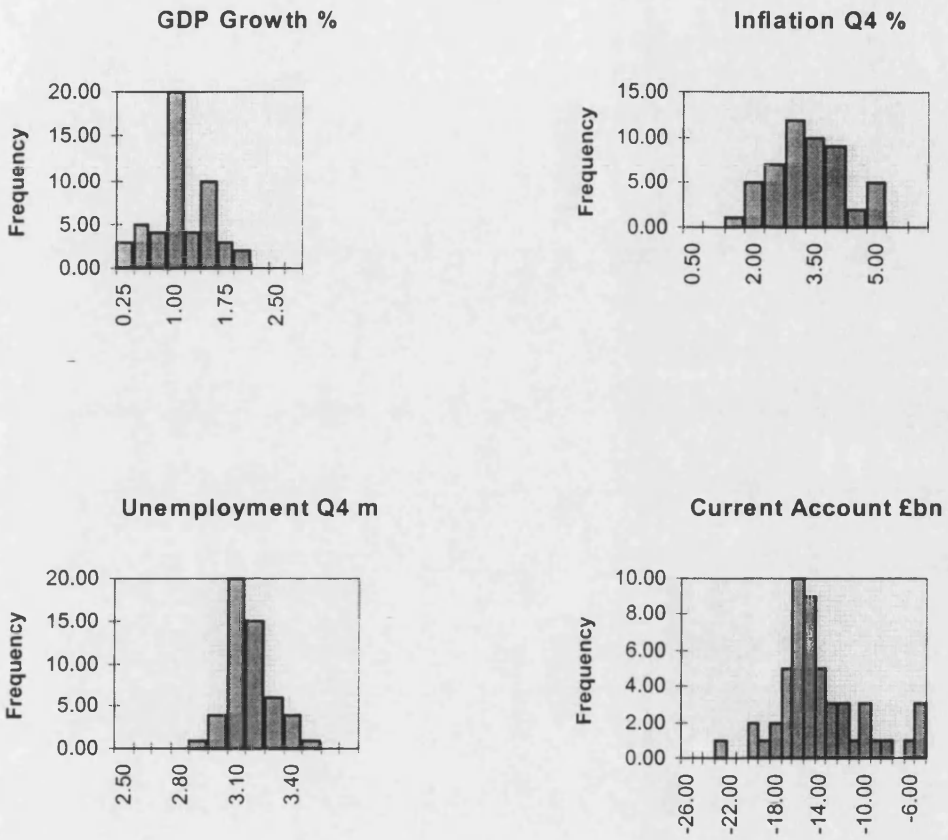
The purpose of this chapter is to illustrate this point and to demonstrate the ways in which the judgement and expertise of economists bridges the gap between an extrapolation and a forecast. To do this, I have used the model detailed in Appendices A and C to reproduce the average of the forecasts made by the Seven Wise Men at their meeting in February 1993. This particular forecast (i.e. the average) is important for two reasons. The first is that it is a proxy for 'the average of recently published forecasts', and this is an important reference point for economic forecasters. Put simply, a forecast which turns out to be wildly inaccurate and stands outside the consensus will make the forecaster look particularly foolish. Of course there are big gains for a forecaster who is the only one to get it right, but this is a high risk strategy. On the other hand, if a forecast turns out to be wrong but is nevertheless within the consensus, things are not so bad as blame (like praise) can be shared. The implication here is that the reference points against which economic forecasters judge themselves, and are judged by others, are other forecasters first and the economy only second. Truly, there is safety in numbers and the herd instinct is particularly strong in economic forecasting.

The tendency for forecasters to converge (intentionally or not) towards a consensus value is illustrated in the histograms shown below. The histograms show forecasts for the 1993 values of four key economic indicators made by over fifty different organisations. All the forecasts were made at the end of 1992.

As can be seen from Figure 2, the forecasts are more or less normally distributed around a mean value. For example, for the 50 GDP forecasts which are summarised in histogram in Figure 2, the mean value is 0.98 and the standard deviation 0.42. What this means is that at any point in time there is a consensus value around which forecasts tend to cluster. One way of interpreting the average of recently published forecasts, is thus as a yardstick against which other individual forecasts can be compared. Forecasts

which are close to this value require less justification and defence than the outlying ones. In this way, the average also defines the consensus - it is roughly what most forecasters are thinking.

Figure 2: Forecasts made in 1992 for key economic indicators in 1993



Because of the importance of the 'average of recently published forecasts' I am going to using my own model to reproduce the average of the Seven Wise Men's' February forecasts. The analysis shows how the need to conform to the pressures of the social group can impact upon and shape economic forecasts and, in the extreme case, can actually determine what they will be. In later chapters I will argue the point in the opposite direction and show how judgements which challenge the consensus values can be used to produce more distinctive forecast positions. In either case however, it is the economist(s) and not the model which is doing the forecasting. Thus, although I have chosen to replicate the average forecast, it would have been possible for me to re-produce any and all of the individual forecasts if I had so wished, and this claim is further substantiated in the following chapters.

The purpose of the present exercise, is to show how, for the orders of magnitude which matter to economic forecasters, the model does not determine what the forecast will be[1]. Throughout the emphasis will be on the thought processes, arguments and judgements which I was forced to make as I compared what my model was telling me with other information available at the time.[2] The chapter thus offers a reflexive and ethnographic, if somewhat personal, account of how an economic forecast is produced.

The February 1993 Forecast(s)

Preliminaries to forecasting

Before I could actually use the model to forecast I had to estimate, compile and debug the program. After all the usual trials and tribulations associated with programming a computer, I thought I had built a working model and so I began forecasting 1993. However, it was clear that my first effort was something less than a success. In particular, the level of the consumer price index, the inflation term used in Keating's model, fell from 114 in 1992Q4 to just 66 one year later, with the result that consumers' expenditure rose very rapidly indeed. Other wildly anomalous results were expected interest rates of over 230 per cent and an unemployment figure of about 5.7 million.

Some of these implausible (even to me) forecasts could be attributed to mistakes in the programming. For example, the formula calculating the expected real rate of interest bore absolutely no relation to the estimated equation from which it was supposed to have been derived. Other smaller mistakes were incorrect signs on some coefficients. However, correcting these mistakes was not sufficient to resolve the problems, although using the correct coefficients did improve the expected interest rate forecast to a rather more plausible 6.5%.

As a result of these, and other changes[3], the model code is now (once again) as 'bug free' as I can make it. There are no errors of which I am aware and so I can once again begin forecasting. The time is February 1993 and the situation in the UK economy can be summed up as follows:

> Since the middle of 1990, the UK has experienced a prolonged and severe recession. GDP fell in 1992 for the second year in succession, and unemployment has risen to about 3 million. Since the Government suspended its membership of the Exchange Rate Mechanism (ERM) on 16 September [1992] it has been able to relax monetary policy by cutting interest rates and the exchange rate has fallen[4].

The challenge facing economic forecasters is to predict what will happen as a result of these changes in economic policy.

A First Forecast

The first forecast an economist would make would simply be to run the model forwards and see what happens. It would not be expected that this forecast would be the one to get published, but the extrapolation would offer some sort of guide as to where the economy might be going if past trends were to repeat themselves. No adjustments would be made at this stage, and very little expertise is required to produce this sort of forecast. In fact, you don't really need a large econometric model to do this sort of forecasting. A simple Vector Auto-Regressive model (in which everything is regressed on to everything else) can provide this sort of extrapolation for much less time and trouble. The problem is, as we shall see, that these extrapolations are not much use as forecasts. In order to turn the initial extrapolation into a plausible forecast the economist has to use his or her expertise to adjust the model in the light of what they know about the economy. In other words, the economist will have to sift and analyse the vast quantities of economic data which are produced daily and use the judgements distilled from this process to finesse the forecast into a persuasive and coherent story. The model may ensure that all the numbers add up, but that is about all it does.

Table 1 (overleaf) shows a selection of forecasts for the 1993 values of several economic indicators which could have been produced in February 1993. The first column shows the Average of the Seven Wise Men's forecasts, as published in their February Report to the Chancellor[5]. The second shows the model-only forecast produced by my model. The third column shows the forecast produced by a Vector Auto-Regressive model, subject to the constraint that GDP growth in 1993 must be 1.2% (i.e. the same as the Seven Wise Men Average). For comparison, the fourth column shows the actual outturn figures for 1993

These forecasts can be ranked according to the economic expertise which was necessary to produced them. In descending order the ranking would be, firstly, the Average of the Seven Wise Men which represents (in some sense) the crystallised wisdom and experience of seven leading UK economists. Second is my own effort, which represents the sort of thing that more or less anyone could do, so long as they know enough about economics and computing to program the model into a computer. Finally, the VAR model requires the least skill of all, as one doesn't have to understand

the first thing about economics in order to use it. In the remainder of this section I will elaborate on this distinction and show why it is that forecasting is so much harder than econometrics.

Table 1: A selection of forecasts for 1993, together with outturn data

	Seven Wise Men Average	Structural Model	VAR model	Outturn Values
GDP % growth	1.1	-1.0	1.1	1.9
Price Index (1993Q4)	3.9	2.8	7.9	2.7
Interest rates %	5.4	6.33	8.4	5.8
Current account (£bn)	-15.7	-10.1	-11.1	-10.7
PSBR (£bn)	47.0	32.8	43.9	45.9

From Extrapolation to Forecast

By comparing columns 1 and 3 of Table 1 we can see that the judgement of the Seven Wise Men is that interest rates and prices will be rather lower than a simple extrapolation would suggest and that the balance of payments deficit and the PSBR will be higher. In other words, we might say that the economists are more optimistic about prices and interest rates than the VAR model, but more pessimistic about net trade and the PSBR.

Another way to look at this difference might be to ask whether or not economic forecasters are conservative, in the sense of tending to underestimate year-on-year changes and over-emphasising the inertia or momentum in the economy. To address this question we need to look at the outturn data for the previous year and compare the changes which have been forecast by each system. In 1992, prices increased by 3.7%, short term interest rates stood at 7.5%, the current account balance was £bn -11.7 and the PSBR was £bn 37.3[6]. We can see therefore that, for the same growth in GDP, the economic forecasters are not consistently more conservative than the economic extrapolaters. Thus, although they forecast less of a change in Prices, they forecast bigger changes in Interest Rates, the Current Account and the PSBR.

With regard to the forecasts produced by my structural model, we can see that things do not look too good. In particular, I am forecasting a contraction in GDP of around 1%, despite the devaluation and other relaxations of monetary policies which have occurred in the preceding few years. For example, interest rates were cut from 15

to 10.5% between 1990 and 1991, and rates had fallen again, from 10 to 6%, in the previous few months[7]. Taken together these imply a considerable boost to the economy which should manifest itself in GDP growth. To try to find out where the model might be going 'wrong', it is necessary to look at the components of GDP. As was discussed in Chapter 2, GDP is the sum of several different categories of expenditure. To understand why the model is forecasting a fall in GDP it is therefore necessary to the examine the individual forecasts for each element of GDP which are shown in Figure 3 (overleaf). In my model, GDP is forecast using the identity given in Equation 1 below.

Equation 1: Identity for GDP

GDP = Consumers' Expenditure
+ Gross Fixed Investment
+ Stockbuilding
+ Government Expenditure
+ Exports
- Imports
+ Net Oil Exports
- Adjustment to Factor Cost

If we look at the forecasts for the individual components of GDP, which are shown in Figure 3 below, it can be seen that the majority are actually increasing throughout 1993.

Figure 3: Model only forecasts for GDP and its components

Forecasts for MODEL2.PRG (i.e. the one with no adjustments)

ENTRY	GDPE	Consumers' Expenditure	Fixed Investment	Stockbuilding
1992:04	134536.00000000	85801.911075366	31801.004613527	2530500.0000000
1993:01	128858.46830563	86091.574708815	32556.474937994	2530646.5658070
1993:02	127918.64186875	86921.726089594	29554.121505269	2530985.8214860
1993:03	129900.50225242	87761.774399129	29133.228947722	2531427.4053428
1993:04	133185.27236814	88667.403850992	30129.930785551	2531922.6789842

ENTRY	Government Expenditure	Exports	Imports	Oil Exports
1992:04	30028.000000000	32925.729388517	36165.000000000	-100.729388517
1993:01	30100.811494758	33773.298662270	35854.533769487	-97.306911365
1993:02	30202.969834087	34602.714937693	35784.505666221	-92.895784248
1993:03	30300.377648518	35420.756194853	35138.231526046	-89.904444952
1993:04	30400.959803183	36234.442110861	34537.021610576	-87.314583375

ENTRY	Adj. to Factor Cost
1992:04	17771.838931040
1993:01	17858.416624360
1993:02	17824.744726369
1993:03	17929.082823690
1993:04	18118.401629843

The only exceptions to this trend are Imports and Stockbuilding. However, Imports are a negative contribution to GDP and so, all other things being equal, a fall in Imports would increase GDP. Consequently, the too-small forecast for GDP cannot be

attributed to the declining forecast for Imports. Although the fall in Fixed Investment does have a negative effect on GDP, and may therefore be to blame for the fall in GDP, boosting the forecast for Fixed Investment to increase GDP is not an attractive option. The reason for this is that, according to the Average of Recently Published Forecasts (i.e. the Seven Wise Men), Fixed Investment is expected to be negative during 1993. Although the range of values is large (ranging from -3.1% to +2.6% for the six which use the same definition) the average value for the forecast for 1993 is -1.9%.[8] Thus, increasing the Investment forecast would position the forecast outside the consensus view and this is something an economic forecaster might be reluctant to do without good reasons.

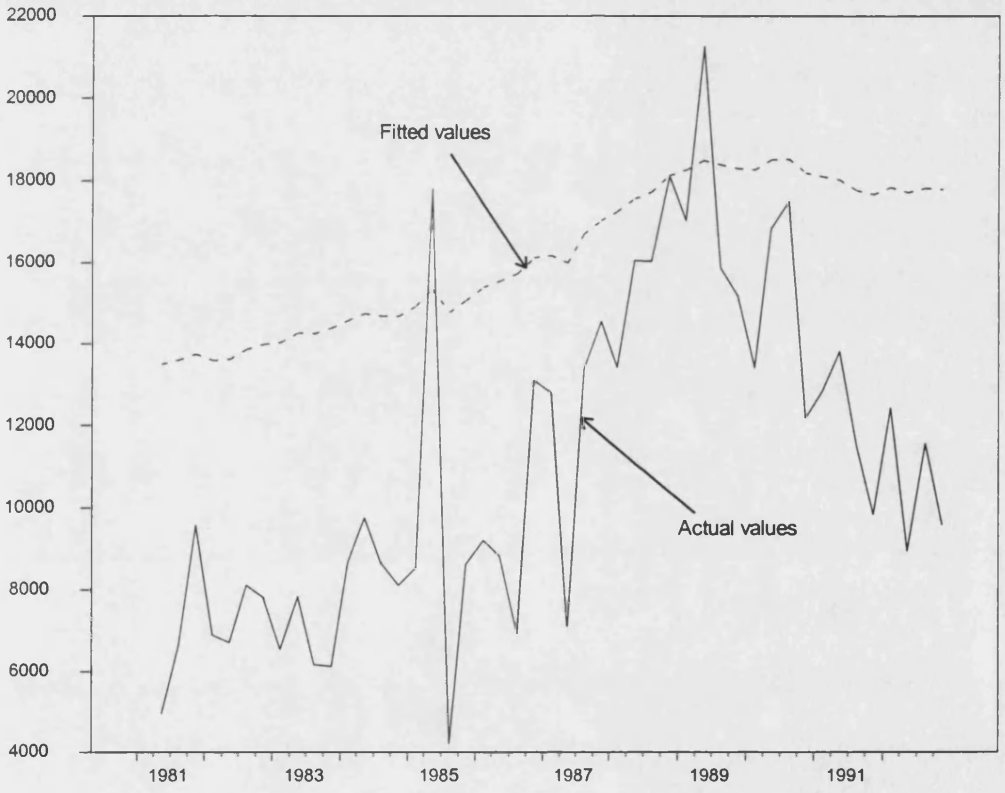
It is interesting to note at this point that the uncertainty surrounding the forecast for Fixed Investment is actually very large compared to the sort of change in GDP which is important in economic forecasting. For example, if, in my model, the forecast for Fixed Investment was adjusted so as to show a growth of 1.5%, which is well within the range being forecast by the Treasury's Panel of Forecasters, this would be sufficient to increase the forecast for GDP from its current value of -1.0% to a growth forecast of 0.6%. Thus, the uncertainty which surrounds the forecasting of one component of GDP is sufficient to swamp the forecast of the aggregate. When evaluating economic forecasts it is therefore important to remember the cliché that a chain is only as strong as its weakest link. In economic forecasting this means that a forecast is only as good as the economist's best guess about the most volatile component.

Returning to the forecasting problem, the decision is taken not to 'fix' the GDP forecast by increasing the Fixed Investment forecast, and so another explanation must be sought. If the forecast is for a contraction in GDP and the components which contribute positively to GDP are all forecast to increase, then it follows that the contraction must be due to an overly large negative contribution from either Imports, Net Oil Exports or the Adjustment to Factor Cost. We have already noted that Imports are falling, so they cannot be the cause. Although the forecast for Net Oil Exports is negative, the numbers involved are very small and, are in any case, consistent with the notion that the UK is a net oil importer (which I think is the case). This leaves the Adjustment to Factor Cost, which is something like a balancing item that ensures the components of GDP sum to the total. Although it is forecast by an equation in the model, it has to be admitted that this equation is not very good. In particular it can be

seen that, at the end of 1992, the fitted value was approximately 8000 more than the actual value (see Figure 4).

Given that this is the case, then a good place to begin adjusting the model, would be with the Adjustment to Factor Cost. In other words, because it is known that this equation has over-predicted in the past (and quite dramatically) it would be appropriate to attempt to offset this known miss-specification with a residual adjustment. By calculating that the fitted value was (to the nearest 100) 8200 too high at the end of 1992, the decision was made to reduce the Adjustment to Factor Cost by this amount for each quarter in 1993. This may not be the final pattern of residuals which need to be set, but it is a good first approximation.

Figure 4: Actual and Fitted Values for the Adjustment to Factor Cost



After thus revising the Adjustment to Factor Cost the forecast for GDP was for growth in 1993 and this seemed more consistent with both the general expectation of other forecasters and also the story being told by the model (which was forecasting growth in most categories of expenditure). Before making further adjustments to 'fine tune' the model, I also noticed that my assumption about interest rates was at odds with the central expectation of the other forecasters.

My initial assumption was that interest rates would remain unchanged throughout 1993. However, I have to ask how plausible is this assumption in the light of what is known about the economic situation in the UK? Firstly, although recovery is forecast, the average of forecasts is still only for below trend growth of 1.1%. The recovery is not therefore expected to be strong. In addition, as we shall see, some of the reasons for the anticipated weakness of the recovery is the belief that consumers' expenditure will be restrained (e.g. because of the debt overhang from the 1980s). Keeping interest rates higher than they need to be will only compound this problem and, because of this, must be regarded as an unlikely economic policy. In addition to its inhibitory effect on consumers' expenditure, a high rate of interest will also tend to push up the exchange rate (by making sterling deposits attractive to investors). However, export growth, which is important for economic recovery, can only be held back if the exchange rate is unnecessarily high.

There are therefore several reasons to believe that the rate of interest is likely to fall during 1993. In the light of this evidence, the decision is made to change the exogenous projections for interest rates to show a constant decline throughout 1993, reaching the Seven Wise Men average of 5.4% in the fourth quarter.

Once these changes have been made the model is run again and a new set of forecasts for 1993 produced. Now that GDP forecast is at least in the right 'ball park', and the exogenous assumptions plausible, the task of forecasting the economy in detail can begin. Given the central importance of Prices in the model (see Figure 1) the first thing to check is the forecast for the Price Level in 1993.

Forecasting Prices

When forecasting Prices for 1993, what one is effectively doing is trying to identify which, of all the factors which cause prices to change are going to be the most important in the current situation. The Panel of Independent Forecasters describe the decision in terms of the balance of two opposing tendencies:

> On the one hand, the fall in the exchange rate since September will inevitably put upward pressure on retail prices in the short term. On the other, the considerable slack in the economy is a powerful disinflationary force ... The extent to which any rise in inflation due to the recent fall in the exchange rate proves temporary will be determined by the behaviour of wages. [The Wise Men] all agree that there is some risk that inflation could

> rise in 1994 as the recovery gathers place. But [they] have different views about the likelihood of this occurring. Most of [them] think that low headline RPI inflation (due in part to recent cuts in mortgage rates) and the high level of unemployment should keep the rate of earnings growth to below 5% in 1994. But [two] expect that these factors will be outweighed by the effects of stronger activity.[9]

It is clear that what is needed is some sort of judgement about how the effects of the devaluation will be offset by the recession which preceded it. In particular, how much will one tend to cancel out the other. This is the sort of judgement which economic forecasters are routinely called upon to make. It is known that there are two opposing forces acting upon the economy. The past data show how things have worked out in the past. The econometric model tells us how things will work out this time, based on an aggregation or averaging of what happened before. What the economic forecaster has to do is to, firstly, recognise the general case (e.g. a devaluation) of which the present circumstances are but an instance. The second judgement economists have to make is to identify the factors which make the present situation unique and to incorporate these into the analysis and forecast.

In Table 1 my forecast was for Prices to increase by 2.8%, which was below the Seven Wise Men average. However, because Prices are, in part determined by previous values of GDP, increasing GDP (which is what the adjustment to the Adjustment to Factor Cost effectively did) means that the new forecast for the Price Level should be higher. In fact the new forecast for Prices is for the Consumer Price Index (the measure used in my model) to increase by 3.9%, from 113.97 in 1992 Q4 to 118.36 in 1993 Q4. Now, this is exactly the same number as the average of the forecasts produced by the Seven Wise Men. However, satisfaction at this outcome is tempered by remembering that, although the numbers may be the same, the forecasts are for different Price Indices. My forecast is for Consumer Price index (which is calculated from the ratio of consumers' expenditure in current prices to consumers' expenditure in constant prices). The Seven Wise Men, on the other hand, forecast Retail Prices. According to the Bank of England:

> The RPI [Retail Price Index] is intended to measure the cost of purchasing a representative basket of goods and services. It includes expenditure on consumption only; expenditures on investment and saving are excluded[10].

However, the RPI, which includes Mortgage Interest Payments as a measure of housing costs is not the measure favoured by the government. The government's anti-inflation policy targets the RPIX measure of inflation, which is the Retail Price Index excluding Mortgage Interest Payments (MIPS). This qualification is important as different Price Indices change at different rates. Thus, whilst the Panel of Forecasters expect RPIX to increase by around 3.9% in 1993, the RPI (i.e. the more inclusive measure of prices) is expected to increase by only 2.6% in the same time period[11]

There is thus an important choice to be made: which of these two measures of inflation is most like the Consumer Price Index forecast by my model. Because consumers' expenditure (the data series from which the Consumer Price Index is constructed) includes 'accomodation in owner-occupied homes'[12] then it seems likely that the Retail Price Index which includes MIPS is the more appropriate proxy. However, at this early stage of the forecasting process, the decision is made that the best strategy would be to hedge my bets and go for an inflation forecast somewhere towards the bottom end of the range marked out by the RPI and the RPIX measures. In order to achieve the desired outcome and restrain the growth in Prices during 1993 to a slightly lower level the following residual adjustments were made:

Table 2: Residual Adjustments made to Price equation

Date	Adjustment
1993 Q1	0.000
1993 Q2	0.000
1993 Q3	-0.001
1993 Q4	-0.002

The result of these adjustments was to reduce the growth in Prices to about 3.5% in 1993. Whilst the initial feeling is that this may not in fact be enough to fully reflect the weight of housing costs in the Retail Price Index, and hence in my Consumer Price Index, it should be remembered that adjustments yet to be made to other equations in the model will also impact on Prices and so the final forecast is still undecided. The adjustments made thus far, like those to the Adjustment to Factor Cost, are best thought of as a first approximation in an iterative process which may have several more loops to run.

If some justification for these adjustments were needed (apart from the excuse that is the safest option) the following reasons could be adduced. As was noted when

making the projections for interest rates, it is generally recognised that any recovery which does occur in 1993 will be weak, and that consumers will be unusually reluctant to spend the economy out of recovery. What this means for Prices is that producers will be unable to pass on the increased costs associated with the rise in import prices due to the devaluation and so 'imported inflation' might be expected to be lower in this cycle than in previous ones. In addition there are grounds for believing that the large pool of unemployed workers will keep a firm downward pressure on wages and so this force, which would tend to increase inflation, is also unusually muted. Taken together these observations suggest that there are several distinctive features in the present economic situation which make it reasonable to believe that Prices will rise by less than an extrapolation of past trends would suggest.

After having thus set an initial and plausible path for Prices, the forecaster's attention next turns to the components of GDP. The first to be considered is Consumers' Expenditure, principally because it is the biggest.

Forecasting Consumers' Expenditure

As can be seen from Figure 3, the initial model-only forecast was for Consumers' Expenditure to increase during 1993 by around 3.4 %. As has been pointed out before, it is generally believed that the economy is at an unusually low ebb and this forecast thus seems rather too optimistic. This is a suspicion which is confirmed by the Seven Wise Men:

> [who all] agree that consumer spending is likely to be restrained by balance sheet problems (for example, the large overhang of debt), the weakness of the housing market, rising unemployment and a squeeze on real incomes caused by rising import prices.[13]

At this point, several options are possible. For example, one could argue that in order to restrain this growth the government will be forced to raise interest rates in 1993 and not to let them fall, which is the current expectation. However, as was already noted, an appreciation of sterling on the foreign exchanges would be very damaging to exports and one of the last things the government would be expected to do. Thus, Gavyn Davies, in his submission to the Treasury's Panel of Forecasters explicitly warns of the dangers of too-high interest rates and argues reducing rates should be the priority, with sterling being allowed to slide if necessary. Patrick Minford and Wynne Godley advocate a similar policy, although for different reasons.

Given this analysis, reducing the forecast by returning interest rates to the higher than necessary values previously assumed does not seem a particularly plausible solution. In addition, if it is consumers' debts which are behind the current high saving ratio, increasing the cost of this debt more than necessary seems a particularly unlikely government policy. In other words, the interest rate assumption seems perfectly plausible and is not therefore a particularly appealing candidate for change.

Another possible way in which the behaviour of consumers might be influenced is through the effect of wages. As noted when forecasting Prices, there is a case for arguing that the large pool of unemployed workers will mean that wage increases will remain low. Although wages do not appear directly in the Consumption Function used in my model, disposable income does. One solution to the problem of overly high consumers expenditure forecasts might be therefore to revise the exogenous projections of disposable income down, in light of the expected low level of wage settlements. At present, disposable income projections are produced by a simple univariate autoregression equation. This is hardly a sophisticated forecasting tool and so the use of a judgmental adjustment to the exogenous projections can be justified. An alternative, would simply be to impose a residual adjustment on the Consumers' Expenditure equation to represent both the unusual reluctance of consumers to spend and also the unusually low level of wage increases which are expected to lead to a relatively small growth in disposable income.

I suspect a professional modeller would first revise the exogenous projections for disposable income to make them compatible with the assumptions and forecasts about wages and only after this to adjust the Consumers' Expenditure equation. Revising the exogenous database has the presentational advantage of not appearing in any list of residual adjustments and also means that any residual adjustments which are necessary will look smaller. In my case, for ease of programming and presentation, I chose to make the adjustments to the Consumers' Expenditure equation itself. The adjustments are given in Table 3 and have the effect of reducing the growth in Consumers' expenditure to around 1%. However, reducing Consumers' Expenditure has also reduced GDP and the lower levels of GDP have fed back into the Price equation, causing the forecast for Prices to fall slightly. It is therefore possible that the residual adjustments made to the Price equation will have to be reset to zero in the next iteration.

Table 3: Residual Adjustments to the Consumers' Expenditure Equation

Date	Adjustment
1993 Q1	-0.001
1993 Q2	-0.007
1993 Q3	-0.007
1993 Q4	-0.007

Gross Fixed Investment

As was noted when discussing the model-only forecasts shown in Figure 3, the range of uncertainty which surrounds the forecasts for Gross Fixed Investment was very large, with some of the Panel of Forecasters anticipating an increase in the level of Fixed Investment and others expecting a decrease. The range of plausible outcomes was such that the judgement made about the level of Investment could mean the difference between a forecast for GDP growth and a forecast for continued recession. Indeed, it can be argued that many of the major forecasting mistakes can be attributed to mistakes in the forecasts for Investment. These problems notwithstanding, a forecast must be made for Fixed Investment and this is the task to which we now turn.

After the adjustments which have been made so far, the model is predicting that Fixed Investment in 1993 will increase by around 11%. This is clearly too high and is well outside the range of values forecast by the Seven Wise Men. Before going in and making a series of residual adjustments however it is important to consider the possibility that the forecast is due to an error in some other part of the model which then feeds into the Investment equation where it then becomes apparent.

In the model, Fixed Investment is determined by the Wages and Employment. It is therefore important to ensure that the forecasts for these two series are plausible before the Investment equation is altered. Because Wages influence Employment, the forecasts for Wages will be considered first. At present Wages are forecast to remain virtually static throughout 1993, with the index of wages and salaries increasing by 0.1, from 115.4 to 115.5.

As was noted when discussing Prices, one effect of the ending of the recession is expected to be an upward pressure on wage demands. On the other hand, it was also noted that high levels of unemployment will tend to work in the opposite direction. In addition, the conclusion that Prices will be increasing at what is an historically low rate should also act to keep wage increases relatively low. However, the recession has ensured that wage increases in the previous few years have been very low, and there is therefore a belief that the growth in GDP which is expected to occur may be sufficient

to trigger an increase in the rate of earnings growth. As a result of these considerations, the range of forecasts for Average Earnings by the Seven Wise Men runs from 2.8 to 6.4%. Thus, although some see Wages as a bigger problem than others, an increase in the level of wages is nevertheless the expectation of every forecaster. In light of this, the current forecast is clearly in need of some adjustment, and it is possible that the equation itself may need re-specifying in a more fundamental manner. However, the immediate priority is to ensure that the forecast for wages is an increase throughout 1993; any re-specification will have to wait until another day. By making the following adjustments to reflect the wage pressure due to the upturn in economic activity, as well as to correct the perceived mis-specification, the forecast for the Index of Average Wages can be made to show an increase of about 4 per cent.

Table 4: Residual adjustments to Wage Equation

Date	Adjustment
1993 Q1	0.000
1993 Q2	0.012
1993 Q3	0.015
1993 Q4	0.020

Having thus achieved a plausible forecast for the increase in Average Wages, we must now turn our attention to the forecasts for Employment, which was also a determinant of Fixed Investment. The current forecast is for employment in the economy to fall during 1993. Initially this seemed quite plausible because the Seven Wise Men expect unemployment to increase by around 300 000, from 2.9 million to 3.2 million throughout 1993[14]. Thus, although the fall is rather large (it actually implies an unemployment level of about 4.5 million if all the newly unemployed are added to the jobless total) at least the direction is correct. In fact, the average of the Seven Wise Men's' forecasts is for Employment to fall by about 2%[15]. Again, the general direction of the forecast is correct and using residual adjustments shown in Table 5 the forecast can be 'fine-tuned' to this range.

Table 5: Residual Adjustments to Employment Equation

Date	Adjustment
1993 Q1	0.022
1993 Q2	0.022
1993 Q3	0.022
1993 Q4	0.018

Having now ensured that the inputs to the Fixed Investment equation are as good as possible, we are now in a position to examine the forecasts for Investment. Unfortunately the changes made have in fact made things a little bit worse. The forecast now is for Fixed Investment to increase by 13%. However, by imposing non-zero residuals, the forecast can be made to show a fall of 2%. This is consistent with the Seven Wise Men's forecasts, which ranged from -3.1 to +2.6 per cent.

Stockbuilding

Like Fixed Investment, Stockbuilding is a fairly volatile component of GDP and forecasting it is made difficult by the general uncertainty which surrounds business spending. The forecast produced by the model is for Stockbuilding to make a positive contribution to GDP growth in 1993. Whilst this is in the right general direction, the actual forecast is for Stockbuilding to increase by an amount which would add just over 1% to GDP[16]. This is higher than any of the forecasts produced by the Seven Wise Men and, in the absence of a strong reason for expecting a high level of Stockbuilding, a risk-averse forecaster would revise this forecast down to something closer to the average.

Table 6: Residual Adjustments for Stockbuilding Equation

Date	Adjustment
1993 Q1	0.0000
1993 Q2	-0.0001
1993 Q3	-0.0001
1993 Q4	-0.0001

This option is doubly attractive as what reasons there are for making adjustments tend to be those which would favour a cautious approach. In particular, the tentative nature of the recovery and the general belief that it is far from firmly established, means that businesses might be expected to be reluctant to invest in Stockbuilding as it is not clear where the return on this investment is going to come from. In the light of these considerations, the following adjustments were made and the forecast for Stockbuilding is reduced slightly so as to show a positive contribution to GDP growth of 0.25% (the Seven Wise Men average is 0.28%)

However, an unintended consequence of this is to make Fixed Investment fall by more than we had wished. The residual adjustments made for that equation will need to be revised before the forecast is finalised. At present, the decision is made to continue with the first iteration of the forecasting process and make sure that the Net Trade

forecasts are satisfactory. Only after the whole forecast is a satisfactory first approximation will the fine tuning of individual components make sense[17].

Net Trade

Net Trade is the contribution made to GDP growth by Exports minus Imports plus Net Oil Exports. The Seven Wise Men expect Net Trade to contribute 0.4% to GDP growth during 1993. For my model, the forecast is very similar, with an initial negative contribution to GDP changing into a small positive one by the end of the year. This is, in fact, pretty much what one would expect to see as a result of the devaluation. As imports become relatively more expensive and exports relatively cheaper, one would expect exports to rise. The problem is that one would not necessarily expect imports to fall, rather the expectation would be that the rate of growth of imports would fall.

Thus, the Seven Wise Men expect that both imports and exports will grow during 1993, but that exports will grow faster, and so give a positive overall contribution to GDP overall. In my model, although the final forecast is plausible (and thus no adjustments have been made) the individual forecasts do not seem to match the expected pattern. In particular, the forecast for imports is for them to fall during 1993. This is completely at odds with the average of recently published forecasts (or at least their proxy, the Seven Wise Men).

The averages of the Seven Wise Men's forecasts (excluding Godley, who forecasts a different variable) are for Exports to grow by 5.5% in 1993 and Imports by 4.7%. The problem is that if these projections are applied to my data values for 1992 Q4, then Imports remain larger than Exports and so the Net trade contribution to GDP is negative. The problem, I suspect lies with the data for Imports and Exports, which show a too large gap between the two. In fact, these series were, as noted in the previous chapter, difficult to obtain and had to be constructed by combining other data series. It is quite likely that some error was made during this procedure. The best solution to the problem would therefore be not to adjust the equation, but to first obtain another data series for non-oil imports and exports against which to compare the ones currently being used. If, as I suspect, there turns out to be a difference, then this should be corrected before any adjustments are made.

For the time being however, the decision is made to do nothing, but to flag the potential problems with the forecast in any presentation. The forecast is thus for Net

Trade to make a small positive contribution to GDP growth and for Exports to grow. Imports are also expected to grow, but at a slower rate than previously.

The forecast so far

After completing the first round of adjustments to the forecast, it is now time to take stock of the situation. Most of the forecasts for the components of GDP are reasonably plausible as a result of the residual adjustments which have been made. The forecast for Prices has fallen slightly as a result of the adjustments to the other equations and now stands at a 3.3% increase over the year. However, as was pointed out above, the forecast obtained for Prices was perhaps a little bit too high in any case, and so this is not a cause for concern.

More troubling is the realisation that the adjustments made in other parts of the model have affected the forecast Fixed Investment, which now shows a bigger fall than was hoped for. It therefore seems that the residual adjustments made to the Fixed Investment equation were too large and need to be reduced. By re-setting the residuals to the values shown in Table 7 below, the forecast is made to show a fall of 1.8%.

Table 7: Residual Adjustments to Fixed Investment Equation

Date	Adjustment
1993 Q1	-0.025
1993 Q2	-0.025
1993 Q3	-0.045
1993 Q4	-0.045

The forecasts for Prices, Wages, Employment and the components of GDP are now in reasonable agreement (whatever that means) with the average of recently published forecasts. The only thing which now needs to be checked is that the forecast for GDP satisfactory.

The forecast for total GDP growth is, in fact rather too high. GDP growth is predicted to be about 3.5% in 1993. As the forecasts for the components are all OK the adjustment must be made in the Adjustment to Factor Cost. At the beginning of the forecasting period, the residuals on the Factor Cost Adjustment equation were set at a constant -8200 through the forecast period. In order to make the forecast for GDP lower, the Adjustment to Factor Cost must be made larger, which means that the residual adjustments need to made smaller. The question is how can this be justified,

other than on the pragmatic grounds that it needs to be done in order for the forecast to come out 'right'.

Fortunately other reasons can be given. If we re-consider the chart shown in Figure 4 we can see that the Adjustment to Factor Cost is a volatile series which does tend to go up and down quite frequently. It is therefore most unlikely that it will remain virtually constant throughout 1993, which is what the current pattern of residual adjustments implies. Moreover, from Figure 4 we can see that the Adjustment to Factor Cost is on a downward trend in the year leading up to 1992 Q4. Consequently, to set a pattern of decreasing residual adjustments for the period 1993 Q1 to 1993 Q4 would introduce a turning point into the series which would be more or less consistent with the previous data. There are some grounds for reducing the residuals on the Factor Cost equation and thus increasing the size of the Adjustment. By setting the following pattern of residual adjustments a forecast for GDP growth of 1.1% in 1993 was obtained.

Table 8: Residual Adjustment to Adjustment to Factor Cost

Date	Adjustment
1993 Q1	-6700
1993 Q2	-6100
1993 Q3	-6000
1993 Q4	-5900

The forecast is now consistent with average of the Seven Wise Men, as can be seen in Table 9 (below). There are still some problems that need rectifying, particularly with regard to the forecasting of Imports. One can hypothesis that had the series for Imports been boosted to a level closer to that of Exports then the negative contribution made to GDP by Net Trade in the first 3 quarters of 1993 would have been smaller and the overall forecasts for GDP growth larger. This is in turn would have implied that residuals set on the Factor Cost Equation might have needed to be set at a lower level.

However, these problems, which could no doubt be solved by further rounds of the adjustment process, should not distract from the main purpose of this exposition. The aim has been to illustrate how an economic forecast is actually produced using a macro-econometric model and to show how important the judgement of the economist is if the forecast is to be plausible. The implications of the example are discussed in the next section.

Table 9: Summary of Forecast for % change in GDP and its Components for 1993 by R. Evans and Seven Wise Men (Average)

	Evans	Average of Seven Wise Men
GDP % growth	1.1	1.1
Consumers' Expenditure	1.1	0.9[1]
Gross Fixed Investment	-1.8	-0.7
Stockbuilding[2]	0.25	0.3[1]
Government	1.2	1.0[1]
Net Trade[2]	0.7	0.4

[1] Excludes Minford [2] Contribution to GDP Growth per cent

Conclusion

The aim of this chapter has been to illustrate the ways in which the apparently precise and quantitative output of an econometric model is the product of a process of interpretation and adjustment. It has been shown that forecasting the economy relies on the skill and reasoning powers of the economists and that the output of an econometric model is not necessarily an economic forecast. Even after the economist has specified and estimated the model, a process which itself is influenced by prior beliefs, the forecasts produced still need to be assessed, interpreted and their plausibility in the wider community established. In fact, forecasting the economy using a macro-econometric model seems to rely even more heavily on the economist's powers of reasoning, rhetoric and judgement than the estimation of the model itself.

One reason for this is that forecasting is necessarily about the future and the future, at least of the social world, is inherently unknowable as existing practices evolve and new ones emerge to challenge and change established relationships and patterns.[18] When an econometric model, however large and sophisticated, is used to project the future the judgement of the economist is a necessary input if the extrapolation of past trends is to become a plausible economic forecast upon which government policies and business investment decisions are to be based. As was shown in the preceding discussion, the exogenous database must be constructed in order that the future paths of the variables which the model needs in order to produce its forecasts are available as inputs. However, these projections are themselves forecasts of the economic future and thus already embody some expectations about what the forecast will be. For example, if

World Trade is expected to decline or grow comparatively slowly then this will have negative consequences for the domestic economy which is going to be exporting into a shrinking market place. Similarly, it is difficult to see how projections for interest rates (which remember are listed as assumptions not forecasts in the reports of the Panel of Forecasters) can be made without having some idea about where the domestic economy is going.

In the forecast presented above, an example of this kind of decision was the treatment of disposable income. Real Personal Disposable Income is an exogenous variable which appears in the consumption function. If consumers disposable income does not increase, or even falls, then this can be expected to have a negative effect on the level of consumers' expenditure and hence to restrain GDP growth or even cause a recession. However, what grounds do economic forecasters have for expecting the growth in Disposable Income to be unusually low apart from the generally weak position of the economy. For example, the large pool of unemployed workers means that wage increases are expected to be low, and if wages do not increase by more than inflation then Real Disposable Income must fall. The result of this is that consumers spend less, thus reinforcing the generally weak nature of the recovery and ensuring that the conditions which brought about the sluggish recovery are reproduced. However, this economic future is exactly what the model is supposed to be predicting and yet it is already contained in the exogenous variables. This is not to say that when setting future paths for the exogenous variables the economist simply sets them in such a way as to produce the desired forecast. Rather the situation is much more like trying to untangle the riddle of whether there were chickens before there were eggs.

Economic forecasters are in effect part of two communities. They are a part of the general social system which makes up the economy and are thus aware of what might be called the 'public mood'. In addition, however, they are also part of a community of 'economy watchers'. Like the 'Kremlin watchers' of the secret services analysing satellite photos, gossip and coded messages, 'economy watchers' spend their lives studying surveys, press releases and prices and from these sources they must produce an analysis of what is going to happen next. The process is thus one of interpretation and uncertainty and, in these situations, intuition and judgement are more useful guides than extrapolation. This is not to deny that there are regularities and tendencies which can be discerned in the economic history of a modern capitalist society. Rather it is to acknowledge what we all know - history does not repeat itself, at

least not exactly. And it is precisely because of this observation that simply extrapolating past trends into the future is not going to be sufficient to produce an economic forecast. The expertise of the economic forecasters is their ability to recognise the patterns and regularities in economic data but also to identify the singularities of the current situation. It just so happens that economic forecasters use a macro-econometric model as a way of integrating this analysis of the general and the particular. We should not however be fooled into thinking that it is the model which is doing the analysis.

It should not necessarily be concluded from this that macro-econometric modelling is somehow not quite scientific. After all, the central message of the sociology of science may well be that all science is based on the negotiation of meaning and evidence. There is nothing unusual going on in macro-econometric modelling. Judgement and interpretation are a central part of all science, although not a part to which much attention is usually given it has to be said.

Notes

[1] The reason I had to do rely on the distilled wisdom of others was because I am not an economic forecaster and do not have either the knowledge or the connections to justify strong and informed opinions. It is this knowledge gap which separates economic forecasters from the general public and explains why a 'ready-to-run' economic model would be of little use to anyone.

[2] For examples of the sorts of adjustments which professional forecasters make see Wallis, K. F., & Whitely, J. (1991). 'Sources of Error in Forecasts and Expectations: UK Economic Models, 1984-8'. *Journal of Forecasting*, Vol. 10, pp. 231-253. The London Business School publication *Economic Outlook* provides a list of the residual adjustments used in its forecasts, although with no description of why they were made.

[3] This chapter is based on paper presented to the Post Keynesian Study Group in February 1995. (See: Evans, R.J. (1995) *How Do They Do That? The Roles of Extrapolation, Econometrics and Judgement in the Economic Forecasts of the Seven Wise Men.*)

Whilst writing up this chapter, I discovered several mistakes in the model used as the basis of that presentation. In particular, the Price equation, which was very problematic in that paper turned out to have been miss-specified in a rather basic way, with log transformations being omitted from two of the independent variables. As the coefficients were estimated using variables in logarithms it seems likely that the poor performance of the Price equation will be improved by returning the model equation to the form in which it was estimated.

Two other mistakes which also became apparent during the writing of this chapter were as follows. Firstly, a constant term had been included in the Consumers' Expenditure equation, although (as we saw in a previous chapter) there are grounds for excluding this if the theoretical coherence of the model is to be maintained. The other error occurred in the Import equation, from which (as noted above) several variables had to be dropped in order for the model to forecast satisfactorily. However, dropping some of independent variables meant that the equation had to be re-estimated and the coefficients in the model updated. This was not done initially but has been for this chapter. The full (up to date) listing of the model code used for this Chapter, which incorporates all these changes, is shown in Appendix C.

[4] *The Panel of Independent Forecasters February 1993 Report.* H.M. Treasury. para 1.

[5] op cit. note 4.

[6] source: op cit., note 4, Table B1, Seven Wise Men Averages.

[7] Andrew Britton, *Submission to Report of the Panel of Independent Forecasters, February 1993*, para. 6

[8] Only 2 of the Seven Wise Men (Britton and Currie) forecast an increase in Investment for 1993. The median forecast is -0.5%. In addition, Minford's forecast, which is not included in these calculations because he forecasts a different measure of Fixed Investment is also negative (-2.7%). Overall it seems that the 'consensus' is negative.

[9] op cit. note 4, paras 12-14

[10] *Bank of England Quarterly Bulletin*, February 1993, Vol. 33, No. 1, p. 12.

[11] source: op cit. note 4, Table B1.

[12] According to a standard economic textbook:

> Consumer goods and services are final products which firms sell to households and which households buy because they are useful or pleasurable. They include items such as bread and ice-cream, shoes and ornaments, haircuts and rides at the fair. They also include acommodation in owner-occupied homes which are held to be let by their owners to themselves.

Source: Parkin, M., & King, D. (1992). *Economics.* Wokingham, England: Addison-Wsley Publishing. Quote at p. 561

[13] op cit. note 4, p. 5

[14] source op cit. note 4, Table 1.

[15] source: op cit. note 4, Table B4.

[16] Stockbuilding is usually measured in terms of its contribution to GDP growth

[17] Does it make sense to think like this about a non-linear system?

[18] The idea is neatly capture in the following limerick:

> A trend is a trend is trend
> But the question is, will it bend?
> Will it alter its course
> Through some unforeseen force
> And come to a premature end?

Source: Cairncross, A. (1969) 'Economic Forecasting: Presidential Address to the Royal Economic Society, July 3, 1969', *The Economic Journal*, **79**, pp. 797-812.

Chapter 4

This chapter complements the 'outsider' perspective given in the previous chapters and gives some idea of how macro-econometric modelling is perceived by those who actually do it. The aim is to illustrate how the ambiguity of econometrics, which was highlighted in the previous chapters, manifests itself in the day-to-day practice of macro-econometric modelling. It has already been noted in Chapter 1 that macro-econometric modelling methodologies can permit a range of theoretical specifications to co-exist, with none being unequivocally superior to the rest. This chapter gives some insight into the reasons for this interpretative flexibility and shows why using econometrics to prove or disprove an economic theory is an almost impossible task.

The chapter is about macro-econometric modelling in the most general sense and explores the conceptual and practical problems associated with it. The problems identified apply more or less equally to all macro-econometric models and the chapter therefore highlights the gap between the scientific image of econometric modelling and its actual practice. As with the rest of the thesis, it is not a denunciation of econometric modelling, but an attempt to re-locate the skill and expertise which economic forecasters undoubtedly possess amongst the social practices of the group.

What is a macro-econometric model

In Chapter 2, I described in some detail how one of the equations in a macro econometric model is estimated using regression analysis. A full scale macro-econometric model is 'simply' a number of these equations which feed into each other before being grouped together to form the accounting relationships and identities which define the national income. So far I have focused on highlighting the ambiguity of the regression analysis which is used to estimate these equations, and shown how judgement and the 'cherished beliefs'[1] of the economics community are used to end the Experimenters' Regress[2] which arises when econometrics are used to test a theory. However, there is another aspect of regression analysis which has not been discussed thus far and this is the statistical uncertainty which needs to be attached to the results before they can be properly understood.

For example, in Chapter 2, I presented several Tables in which the co-efficients in regression equations were given to 9 decimal places, which certainly looks very precise. However, this sort of precision is quite illusory when interpreted within the

context of what a regression analysis actually means. At its most basic (i.e. when there is just one independent variable being used to predict the dependent variable) regression analysis calculates the equation for the straight line which best fits the available data points. In regression analysis, 'best fit' is taken to mean that the straight line given by the estimated regression equation is the one from which the sum of the (squared) deviations of the data is as small as possible. If two independent variables are used to explain or predict the dependent variable, then regression analysis calculates the 'plane of best fit'[3]. Although it becomes increasingly difficult to visualise what the regression analysis is doing as the number of explanatory variables in the equation increases, the underlying principles remain the same. In every case, the regression equation is the best estimate of the relationship between the independent variables and the dependent. This means that, if the regression equation is used to predict values of Y_i, given values for the independent variables (X_{1i} ... X_{ni}), then the answer will not be the observed value of Y_i but a number 'close' to Y_i. If the estimated regression equation is used to predict all the values of Y_i associated with all the input data then, on average, the difference between the predicted and actual value will be zero (this is why regression estimators are said to be unbiased). However, it is important to remember that for any particular value of Y_i it is most unlikely that the value of this deviation or error term will be zero. Thus, for each regression equation there is a set of error terms given by the difference between the actual value of Y_i and the values estimated by using the equation.[4]

The practical implications of this are as follows. The apparently precise appearance of a regression equation in which co-efficients are calculated to 9 decimal places is potentially misleading because there is an extra term which has not been shown. This extra term is the error term and can be thought of as a random 'draw' from the distribution of possible error terms. The error could be either positive or negative and the range of values which it is likely to take is determined by the closeness of the fit between the estimated values of Y_i and the original data. In the best cases, these errors will be small relative to the value of Y_i, in the worst they will be of the same order of magnitude.

In other words, every 'point forecast' produced using a regression equation comes with the following implicit caveat: 95 times out of 100, the observed value of Y_i will not be the one calculated but will be somewhere in the range given by the estimated value plus or minus twice the standard deviation of the distribution of the error term (i.e.

the difference between the observed and estimated values). The information value of the forecast must therefore crucially depends on the size of the standard deviation of the error term relative the dependent variable. It is important to remember that all econometric equations have this property - it is a necessary consequence of the statistical methods by which they are produced. The following section discusses this statistical uncertainty in more detail.

Statistical Uncertainty in Macro-econometric Modelling

A macro-economic model is a system of linked equations. Some of these equations are accounting or definitional identities which are true by fiat, others are behavioural equations which attempt to model the actions of economic agents in the economy. These behavioural equations can either be estimated individually and then combined into a whole model, or the whole model can be estimated in one go using a 'systems technique' such a three stage least squares. The 'systems techniques' are in theory the best way to estimate a multi-equation model because the method takes into account the co-variances between equations caused by the fact that dependent variables in some equations appear as independent variables in others.

In addition to recognising the simultaneous nature of the equations, using a systems estimation technique would enable the standard error of the model to be calculated. The standard error of the model is basically similar to the standard error of an individual equation. If it were known, it would mean that any point estimate could be accompanied by an accurate estimate of the range of possible values implied by the fit of the model to the data. This would mean that the statistical uncertainty attached to the model would at least be known and could therefore be used when interpreting and presenting economic forecasts.

If the equations are all estimated separately and only later combined into a single model, then the model standard error cannot be calculated directly and must be estimated by some other means. In practice this is generally what happens and almost all macro-models are composed of equations estimated individually using partial information techniques such as instrumental variables[5]. Because of this a direct measure of the model standard error is not available and so, if one is needed, it must be calculated indirectly. The usual way of doing this is to conduct what is known as a 'stochastic simulation' in which the error term in each equation is allowed to take the range of values specified by the equation's standard error. For each error value, a

simulation run is made and the forecast recorded. By repeating this process for literally thousands of runs, the distribution of forecast values can be plotted and the error associated with the model estimated from this.

From simulations such as these it transpires that the standard errors, however they are calculated, tend to be of the same order of magnitude as the actual outcomes, and the presentation of forecasts to one or even two decimal places is therefore somewhat misleading. For example, a forecast for the GDP growth rate over the next 12 months is surrounded by an uncertainty band of about 1.25% either way. In other words a forecast for GDP growth of 2.5% over the next year really means that it is most likely that the outcome will be between 1.2% and 3.8%, but there remains a 5% chance that it will be above or below this range. When looked at this way it seems that economic models do not tell us very much, especially over the longer term, and this is indeed the case:

> *Britton* after 2 [years] I think you've got to the stage where the information content of the forecast is probably about nil. In other words, that the standard error in the forecast is as large as the standard deviation of what you are trying to forecast, whilst over one year you roughly halve it, which is clearly useful[6]

Once the uncertainty which surrounds an economic forecast becomes apparent it is also clear why despite their theoretical differences, macro-econometric models are all more or less compatible with the economic data. A wide range of outcomes are compatible with any given forecast, and 'the differences that are often argued about are really well within sampling error.'[7]

In fact, a closer analysis reveals that the information content of a pure model forecast is even less than that which has been implied so far. The reason is that the stochastic simulations used to estimate the standard error of most econometric models do not draw out the full range of possible errors. For example, when stochastic simulations are carried out, the coefficients of the equations are usually held constant, whereas they are actually values from a probability distribution and should therefore also be allowed to vary. On the other hand, there are some evidence to suggest that the estimates produced by stochastic simulations are rather too large, particularly for longer time horizons.

> *Currie* I'm less sure about that [i.e. the error estimates produced by stochastic simulations], because, some work I've seen by David Hendry, suggests

actually that the errors don't explode in that way, that they go up and then sort of stabilise[8]. But stochastic simulations are one way of trying to get at it, but even that's not a very good way of getting at it, its not the real answer[9].

In addition to the statistical uncertainty associated with the estimation technique and estimated by stochastic simulations there is another and far less tractable source of uncertainty which arises when macro-econometric models are used for forecasting. When the model is being used for forecasting, as opposed to the analysis of a historical data set, the future values of any variables which are not forecast by the model (i.e. the exogenous variables) must be assumed by the modeller. These exogenous projections add a further unquantifiable uncertainty to the forecasts. However, this is not the only way in which the unquantifiable uncertainty of human judgement enters into economic forecasting. As was shown in Chapter 3, forecasters can fine tune their forecasts by setting the error terms in various equations to take non-zero values and, research by the ESRC Macromodelling Bureau, suggests that such practices are fairly commonplace[10]. Whilst these adjustments usually reduce the final forecast error, which is probably a good thing, they mean that even if the model standard error were known we would still not be able to assess the 'uncertainty band' which should be placed around any given economic forecast because so many other sources of uncertainty had been left out. Interestingly the feeling amongst macro-econometric modellers is that, because the standard error estimates which would be generated if all this uncertainty was fully acknowledged are so large, they are not worth publishing at all:

Ormerod Well, in fact, its been technically feasible to calculate the mean and standard errors for many years. In fact, we could have done this at the National Institute in the mid 70s, but we suppressed it on the grounds that the standard errors were so large, that it would have been difficult for non-specialists, you know people using the models, using the forecasts, to appreciate. It would have discredited them.[11]

Minford The trouble is that these stochastic simulations they have a very restricted bunch of errors that they draw on and you've got the, you have the errors in the exogenous variables as well which are not generally included in these stochastic simulation exercises

Evans And if they were the outcome would be even worse

Minford Oh absolutely, that why its absolutely pointless to publish these forecast error bands because they are extremely large ... I'm all for publishing full and frank statements but you see the difficulty [with] these standard errors is that they're huge. If you were properly to draw out the uncertainty surrounding a forecast it's huge, absolutely huge.[12]

However, it would be quite wrong to conclude from this that macro-econometric modellers are engaged in some sort of plot to conceal information about the uncertainty surrounding an economic forecast. Rather the emphasis is on how the error can be communicated in such a way as to strike a balance between full disclosure and undermining one's own credibility. Apart from the technical criticisms mentioned above, probably the most important failing of stochastic simulations is that they take no account of the input of the modeller. This aspect is particularly important because the ESRC Macromodelling Bureau's *ex post* analyses have frequently shown that the residual adjustments made by the modellers have tended to offset the error in the pure model forecast and improve the accuracy of the published version[13]. In assessing the uncertainty surrounding a forecast, some way of accounting for this 'added value' needs to be found. The most frequently mentioned compromise is therefore for the modelling team to look back over its own past record.

Britton Well I think the best way to do that is to just look at your own past mistakes.[14]

Wallis the best you can do, I feel, is to look back at what your forecasting record has been in the past, obviously it will slowly change over time but hopefully only slowly. I would argue that a lot of the other forecasters ought to do what the Treasury does and report some summary statistics about past performance so that the point forecasts are taken with a grain of salt.[15]

And, in fact, this is what most forecasters actually do. The LBS includes in the *Economic Outlook* details of its previous 4 forecasts; the National Institute forecasts contain a box detailing the mean errors of its one and two year ahead forecasts for a selection of key variables. Another approach might be to discuss a range of outcomes in the text[16]. The common feature of these strategies, and their advantage from the forecaster's point of view is that they evaluate the finished forecast (i.e. the final product) and not just the model (i.e. the machine which is partly responsible for it). However, this does have the effect of changing the question being asked: it is no longer a question of what are the confidence limits surrounding this forecast; but how close to

the outturn has this particular combination of model and modeller been in the past[17]. These problems would remain even if the standard error of the model would be known precisely as some the input of the modeller would still have to be taken into account. In other words, so long as modellers make residual adjustments and exogenous projections, uncertainty bands will remain uncertain.

Who knows?

Given the importance of the 'uncertainty bands' which could, and perhaps should, be drawn around an economic forecast it seems pertinent to ask how far do economists go in drawing attention to these issues. The (stereo) typical individual is perhaps unaware that the apparently precise figures quoted in the media are, in fact, merely the mid-points in a very large range of probable outcomes[18]. However, it remains the case that models are specified and estimated for a particular person and usually at the request of a specific funding organisation. The target audience influences both the style and content of the published results[19].

> *Minford* in the early 80s people who took notice of our inflation forecasts made a lot of money on gilts, which was nice for them, and very important. You've got to remember who are the customers for forecasts. There are two major customers, the city and to a much lesser extent as a matter of interest, business. Business is not that interested in forecasts interestingly. They use them a bit like wall paper, you know to feel respectable ... but the City people want forecasts the whole time.[20]

In other words, the main customers are Business and the City. This is especially true for those forecasting groups receiving little or no public money (which means just about everyone apart from the National Institute) and the difference this makes is evidenced in the following remarks

> *Britton* So [there] was an attempt to become more open, more formal I suppose, and we got support in that way from the ESRC, who took over the financing of model-building here and also at the LBS, and one or two other places. The fact that we were doing it for a Research Council, instead for a government department had a lot to do with the way in which it was done, and particularly the way it got written up. It became more academic, a bit more scientific, and rather less 'Believe me, I know'.[21]

> *Minford* What audience are we trying to convince about these models? Whose beliefs do we weigh the highest in this business? People who actually put

their money in support of them, or academics who don't really give a damn about them?[22]

In addition to the more obvious difference in orientation[23] the existence of private funding groups also creates areas of privileged information, known to some subscribers but not to others. For example, in explaining why the LBS publishes the values of residual adjustments with no explanation of either the specification of the relevant equation or the reason why the adjustment was felt to be necessary, I was told:

Currie Well it would be nice if one could. I mean we tend not to because in that publication [*Economic Outlook*] because its not aimed at model users. We're writing for people who want to know our views on where the economy is going, [and] we happen to have used a model as a technique for getting at it. If we were writing for model users we clearly would describe those things, and indeed when we write up our material for our forecast consortium, who use our model, we do actually provide a description of the variables, why key residuals were set and how the forecast may be sensitive to variation in them. But that's because they're model users - we're not targeting the E[conomic] O[utlook] at that group. [24]

Social Change and Macro-econometric Modelling

The previous section highlighted the ways in which the statistical uncertainty associated with regression analysis means that any given economic forecast is compatible with a wide range of data. This is a proposition with which most economic forecasters would agree and it is something which they seem to take for granted. Although it may seem to an outsider that such uncertainty might invalidate the whole enterprise it seems that the increase in accuracy brought about by the forecasters' adjustments and fine tuning is sufficient to make the funding of the joint project (i.e. model plus modeller) worthwhile.

Moreover, if the unavoidable statistical uncertainty was the only reason why economic forecasts were only ever approximately correct then the continued funding of model development would eventually reduce this uncertainty (e.g. as the sample size gets bigger) and thus, over time, forecasts would become more accurate. The parallel here is with weather forecasting in which a continued investment in meteorological research has improved the accuracy of weather forecasting. Unfortunately, however, the economy is a social system which means that, unlike the weather, the processes which generate the data can and do change over time. Thus, meteorologists are always trying

to model the same system (even though their understanding of it firstly as a deterministic and now as a chaotic system has changed). In the case of economic modelling this is not the case because social systems can change over time.[25]

The following sections detail how economic forecasters resolve the tension between their implicit assumption that the processes which generate economic data do not change (much) over time and their admission that the economic environment is altered by policy and world events. The situation is quite unlike weather forecasting, where more data reduces uncertainty. In economic modelling, where more data can mean more changes to the system, more data can actually mean more uncertainty. The discussion begins with a brief example illustrating just how sensitive econometric models can be to changes in the data set used for their estimation.

Practical macro-econometric modelling

On one of my first fieldwork visits I arrived slightly early, and found the economist I was due to interview working with a colleague on the re-estimation of his model. It seemed that one equation, for a series called 'SSR', was causing particular problems. In the newly estimated equation, the coefficient on one variable was given as 0.6, but the economist felt it ought to be nearer 1[26]. Initial attempts to solve the problem were made by the re-estimating the equation using a different statistical technique. However, because this involved removing the correction for correlation between the error terms, the re-estimated equation failed to satisfy the most basic econometric tests[27] (the Durbin-Watson statistic being too small). Following this, several further attempts were made to resolve the problem by re-specifying the equation by adding new variables and changing the lag structure[28] but none of these strategies was very successful either.

At this point the economist changed the equation back to its original formulation in order to explain to me in more detail what was going on. This time the estimated coefficient was just over 1, exactly the value it 'ought' to have been. A closer inspection revealed that this equation was in fact slightly different from the original version. Although the specification was exactly the same, the data period over which it had been estimated now began in 1970, and not 1973 as in the original case. These extra observations had been enough to bring the wayward coefficient back into line.

This example illustrates one of the central dilemmas faced by all economic modellers; namely, how sensitive are the equations used in econometric models to

changes in the sample period over which they are estimated? Although in the case quoted above it was pre-1973 data which made the difference it is equally possible (as was shown in Chapter 3) that future data will have a similar effect. If this is the case, what warrant do we have for basing economic policy on extrapolations of past relationships?

Although these concerns are well known to economists, perhaps receiving their strongest expression in the Lucas Critique[29], economic modellers seem relatively unmoved by this seemingly fundamental flaw in their approach. Basically the criticism is that parameters (i.e. coefficients) in an econometric model are a function of the policies enacted during the period over which the model has been estimated. If the model is then used to evaluate a new policy the parameters must be changed to reflect the effects of the new policy. However, it is just these effects that the model is supposed to be predicting[30]. In principle the Lucas critique should apply to (and undercut) all macro-econometric model-based policy evaluation. However all econometric modellers I spoke to were keen to minimise its significance.

Minford We obviously have to accept that the Lucas Critique applies to the models. We use them with our heart in our mouths, because obviously we're really betting against there being some change in the environment having mucked up our parameters. We're doing this the whole time, its very judgmental[31]

Evans So you don't think it [the Lucas Critique] applies to your kind of model?

Godley I don't think it applies to it at all. Not seriously at all.

Evans Even though according to Lucas all the behavioural equations in your model would change as a result of the policy change?

Godley Well I don't think they would.[32]

Currie I think the answer is, if one is concerned with that, as we are, is to say well let us look at other methods of modelling which take account of structural change, and there are quite a lot of modelling methods, for example Kalman Filtering which allows one to deal with shifting coefficients over time.[33]

Britton I think that it evolves rather than changes abruptly and its based on a sort of weighted average of the recent past. The recent past might be 20 years in some cases so I don't want to exaggerate the extent to which things change .. The economy won't change overnight; unless we have a revolution its not going to become a totally different economy. So the model is nearly right all the time, but its not exactly right.[34]

This is not to say that the economists I spoke to were in complete agreement about the stability of the economic system. Rather, the issue was how quickly do the processes which generate economic data change over time. If the economy stays more or less the same, then model parameters should remain reasonably constant over time. If however the economy changes rapidly then macro-econometric models should likewise change as they are re-estimated for new data, and perhaps change quite dramatically. Thus even within the conceptual space where econometric modelling can be seen as worthwhile, different methodologies are possible. There are those who believe the economy is fundamentally stable and that models should likewise be similar over time; and those who attempt to match their model to the constantly changing economic environment.

'Strong' and 'Weak' claims in Macro-Econometric Modelling

An alternative way of thinking about this 'slow-change/quick-change' distinction is in terms of the relative weights given to economic theory and economic data. Those economists who believe that models should remain stable tend to be those with a strong commitment to a particular theoretical understanding of the economy. Proponents of this perspective are engaged in what I term 'strong' macro-econometric modelling[35]. In contrast, those economists who allow the economic data to play a greater role in determining the specification of the model can be regarded as adopting a more pragmatic approach and being less attached to a *particular* economic theory. These economists are engaged in what I will term 'weak' econometric modelling.

Taking first the 'strong' perspective (according to which the processes which generate economic data change only slowly), the criticism made of the more data-driven methodologies is that changes in the economy are neither as great nor as frequent as 'weak' models suggest. For example:

Ormerod If you track an individual model over time, the results which you get out of it vary enormously, which must further undermine their credibility ... In any individual model, the multiplier will often vary from say 1 to 2 in a matter

of a couple of years. Now, if that's the case then I simply don't believe what comes out of the model. That can't be right, that it can vary so much.[36]

Minford I think it's just the properties of their model are far too influenced by little wiggles in the data. They don't think through what it is they are trying to say, and have a stable theoretical framework, so you actually know where they are coming from.[37]

In contrast to this pragmatic approach, 'strong' econometric modelling assumes that most of the variation in economic behaviour can be accounted for by the error terms in equations. The theoretical understanding, and hence the actual specification, thus remains relatively stable, as explained below:

Minford My starting point for this is that economics is perfectly obviously not a precise science in the normal sense of the word. It is highly stochastic: relationships are very hard to pin down precisely, and they shift because of all problems that Lucas drew attention to. Nevertheless, there are certain fundamental laws, basically the laws of supply and demand, which regulate economic behaviour, particularly over the long term.[38]

In other words, the stability of the economy is mirrored in the constancy of the model. It follows from this that a good econometric modelling methodology is one in which the structure of the model remains constant over time:

Minford What I'm saying is that we have, subject to re-estimating on quarterly data which we did a few years ago, preserved the structure and just changed some of the parameter detail. We've not felt it was worth changing model parameters for the sake of a few wiggles in the data. The only basis on which we change model parameters is if there are major upsets to our forecasts or some sort of major evidence that the parameters are going wrong.[39]

In contrast to this belief in a clear and stable economic structure, other macro-econometric modellers are equally adamant that changes do occur. Forecasting is only possible because the economic system will change only gradually and thus the forecast period will be sufficiently similar to the recent past for the model to hold as a reasonable approximation to reality. However, because these gradual changes are occurring all the time, frequent re-estimation, and if appropriate re-specification, of macro-econometric models is both a necessity and a virtue.

Britton I was surprised for a moment when you said that the trouble with the Institute model is it changes too often because in some senses the trouble with the National Institute model is that it doesn't change often enough ... I mean, the economy changes a lot and that's the basic difficulty of economic forecasting as compared to say weather forecasting.[40]

Within this pragmatic econometrics tradition the aim is not so much to develop a single 'true' model but rather a portfolio of analogues against which events and changes in the real economy can be compared. However, because econometric models are only informative to the extent that history is repeating itself, novel events are extremely problematic, and it is in circumstances where no comparison can be made that a macro-econometric model will be of least use. Thus

Wallis The place where you're most susceptible to find the model incorrect is where the nature of the change is completely new and has no analogy before.[41]

To sum up what has been said so far: the degree to which the processes generating economic data change or remain the same is one of the key issues in macro economic modelling, and I have used it to make a distinction between 'strong' theoretical models and 'weak' pragmatic ones. However, although I have used this idea to identify different methodologies, it is important to remember its salience for economic policy in general and this is the burden of the rest of the paper.

In the following 3 sub-sections I outline the main results of the interviews undertaken and the remainder of the paper is structured as follows. Next, I expand on the distinction between theoretical and pragmatic methodologies and discuss the differences between the 'weak' and 'strong' approaches in more detail. This section also discusses how the use of formal models adds a scientific *imprimatur* to the forecasting business and sheds some light on the rather vexed question of what economists actually agree about.

The remainder of the discussion looks at the positions adopted by the macro modellers more critically and examines how closely they live up to their scientific aspirations. The analysis investigates issues relating to falsification, and examines what persuades economists that their model needs changing. The main foci here are *ad hoc* responses in hypothesis testing and forecasting as a crucial experiment.

The previous section has already examined the stochastic properties surrounding economic models and forecasts. Taken as a whole, this chapter demonstrates the not inconsiderable diversity of macro-econometric models and theories which are at the policy makers' disposal, and argues that none appears to be significantly better than the rest. Moreover, the possibility of ranking models must remain distant as the most basic questions (e.g. when has a forecast mistake occurred) cannot be answered.

How is Macro-Econometric Modelling 'Scientific'?

On the whole macro-econometric modellers do try to live up to the traditional norms of science, and believe that hypotheses should be tested against empirical data whenever possible. The mainstream view of economic modelling as conforming to traditional science values is well illustrated in the following quote which contrasts the objectivity of scientific forecasts with the subjectivity of what would otherwise be no more than 'assertions of faith'[42].

> *Britton* The feeling certainly was that the Institute was likely to attract more support from the academic community, and would actually perform a better function in contributing to the debate, if it made its approaches as scientific as they could be. Therefore everything we do should be capable of replication - anybody who wants our database can have it; anyone who wants to run our model can have it; and we ought to give statistical criteria by which we'd decided what to do, rather than just saying 'It felt like that'.[43]

The other important science value highlighted in this quote is that of replication. Although replication in all sciences is frequently problematic[44] it raises particular problems for macro-econometric modelling and forecasting. The reason is that, for an experiment to be 'replicable', the skills and tacit knowledge essential for its success must be diffused amongst the community. In terms of macro-econometric forecasting this means that the 'model' must be made separate from the 'modeller'. In other words, for replication to be possible the econometric model must appear as uncontroversial to economic forecasters as a voltmeter would to an electrical engineer. After all, if this were not the case then how would replication be possible?

To be more specific, the problems these issues raise for economic forecasters are twofold. Firstly, the model, far from being a 'black boxed' technological artefact, is the subject of long running controversies both about what sort of model should be used and what even the best model can achieve. Secondly, the techniques used to test hypotheses

in econometrics do not appear powerful enough to resolve any of these disputes. Because of these factors, econometric modelling remains in a state of flux and seems likely to do so for some considerable time. The rest of this chapter outlines the fault lines which run across the forecaster's map of the economy.

'Strong' and 'Weak' Econometrics Again

In the previous section a distinction was made between the 'strong' and 'weak' macro-econometric modelling. A flavour of the 'strong' perspective, which is closely related to traditional science norms and methodological ideals, was hinted at in the extract quoted above[45], A 'strong' macro-economic model would encode the actual rules of behaviour or laws of economics which govern economic activity. The reason is that 'if there are any statistical relationships it must be due to something happening out there in the real world.' (Britton, p. 25) 'Science' is seen as providing the methods by which theories about the world can be tested and false ones discarded. This, in turn, explains the importance and popularity of regression analysis[46] in macro-econometric modelling. By specifying a regression equation it is possible to establish whether or not the behaviour of one variable is likely to have been associated with changes in the behaviour of another[47].

> *Currie* I don't think there is any alternative. I mean all we're doing really is testing hypotheses in a systematic way. If we want to, say, test whether Tim Congdon has something useful to say, well you can take the various parts of his proposition, and test them against empirical [data]. Its the only way you can have a progressive methodology in this area.[48]

Of course, chance correlations and nonsense regressions are always possible. Nevertheless, a clear implication of the 'strong' econometric methodology is that there is only one 'true' model of the economy, and that the purpose of economic modelling is to identify these elusive statistical relationships. If these relationships hold over long periods of time, surviving repeated falsification attempts, then they will be important not just for forecasting but also policy making.

This is not to say that developing an economic model and using it to produce forecasts is a straightforward or easy task, but it is to say something about the sorts of problems which will be encountered. Principally, these will be technical problems associated with the limited amount of data available, the quality of this data, and so on. Another implication of the 'strong' approach is that although judgemental adjustments

(i.e. the imposition of non-zero error terms to individual equations) must be made if and when necessary, they are generally used as a way of incorporating information about policies due to be enacted or as a temporary way of making up for the shortcomings of the model[49]. In other words, they are not a central part of the forecasting process but a necessary evil.

In contrast 'weak' econometric modelling is characterised by two elements. The first of these is that judgement is an important part in the whole forecasting process and not something added on at the end to 'fine tune' the forecast. Models do not supplant judgement but externalise some of the steps taken by the modeller and preserve the structure of the overall argument. With the model now acting merely to ensure consistency, the model and the modeller now become complexly interwoven[50], and the previously clear split between the model and modeller is blurred or even dissolved.

> *Britton* I think there are really two rather different approaches. One is to say that this is a branch of science and that everything must be based on objective criteria which people can understand. The other is to say that is just too inflexible, and that there's something called judgement, intuition if you like, which has its place in the sciences and that its the people who are intuitive who are successful. There's some truth in both I guess[51]

Within the 'weak' methodology, there are those who see the model-modeller split as, at best, a heuristic device:

> *Currie* I suppose the first thing to say is that each of us has in our mind a sort of way of thinking about the world which may or may not correspond to the model's views. I mean, clearly there is some relationship, because we've helped to develop them.[52] (p. 3).

and others who see it as wrong in principle:

> *Godley* I would disagree. I don't, the forecasts I publish are not the result of a model working independently of my mind.[53]

The pervasive influence of judgement in 'weak' macroeconometrics means that replication, a difficult enough task in the first place, has now been rendered virtually impossible. If a forecast is now something like a well-reasoned argument deploying empirical results where appropriate, it is clear that while we may come to agree with the point of view being expressed we cannot replicate its production[54].

The second characteristic of the 'weak' program in econometric modelling is that it puts limits on the range of application of the econometric models. It has already been noted that all models are limited because they can only be used for those circumstances in which the current or expected variation in a particular data series is no greater than that which has been experienced in the past[55]. However, models are further limited by the fact that they have been commissioned, specified and estimated for a specific task. There is not therefore a general macro-econometric model:

> *Wallis* No, there is never a true [model], but there is an economy out there and we have a lot of different representations of it, and approximations to it.[56]

Parenthetically, we can note that from the 'strong' econometric perspective, questions about the generality or 'truth' of a model are also problematic:

> *Minford* I don't know what a true model is ... the word 'true' model is not really very helpful. I'd say what is a useful model for the purpose in hand? Is it a model that on the whole gives you reliable results, for a particular purpose, given the state of technology?[57]

However, within the context of a clearly defined question or purpose (e.g. forecasting main economic indicators and analysing policy) proponents of the 'strong' perspective must surely believe that the veracity of some models will be better founded than others.

Returning to the 'weak' approach in macro-econometric modelling we can also see that the kind of economy which is being modelled is also very different from the one which 'strong' program econometricians are dealing with. From the perspective of 'weak' econometrics, the economy has a shifting, ephemeral quality which means that econometric models must be used with considerable caution. In particular, modellers must continually ask whether their model is still a valid approximation to the economy, or has change rendered it obsolete. The precise timing and nature of any change may not be predictable, but the economic forecasters must always be looking for signs that a change has occurred so that its effects can be taken into account:

> *Ormerod* To say that the system will change, [and] may change at any point in time, means that that's very hard to anticipate. But I think that macro models really help to destroy thought. People should be thinking about these sorts of thing all the time, and I don't think its an accident that people like Congdon and Godley who don't have, quotes, 'elaborate' models which

back them up, spend more of their time thinking about what are the key issues *now*.[58]

In addition, the goals of the forecasting exercise are now rather more subtle than the deterministic futurology implicit in the 'strong' program:

Godley I think tables of numbers are the enemies of good forecasting. [Forecasts] should be judged by whether or not they give a good idea of what the whole situation is going to be like, what character it will have, otherwise you wouldn't bother to write anything[59].

Is there a Consensus?

In the preceding section it was noted that economic models are designed for specific purposes and that this, to some extent, can account for differences between them. However, similarity and difference are often matters of perspective. For example, the Treasury Model of the economy differs from many others in that it has an unusually detailed representation of the public sector, because this is an area of the economy of particular interest to the government. On the other hand, the Treasury model of the economy is also a fairly standard econometric model, employing the same set of economic theory, data and methods as other 'mainstream' models.

All the economists I interviewed were engaged (or had been) in modelling the macro-economy with the aim of evaluating economic policy and producing short- and medium- term forecasts for the major economic indicators. To this extent all the models had been developed for the same task, and differences between them cannot be explained away in this manner. It might be expected, therefore, that there would be a considerable degree of similarity between the models. In this section I examine what the modellers see as being the areas of similarity and difference between their models.

The basic claim for similarity between macro-economic models is that they all have the same 3 major relationships, and that disagreements are over orders of magnitude, lag structure and other differences of degree.

Minford The competition we're talking about here is competition very much at the margin, in trying to, its almost like different sorts of camera, whether they catch the light in a more or less appealing way. All the features are very similar.[60]

Wallis In general there is a kind of consensus, a general framework which most people subscribe to. The argument really is about orders of magnitude,

relative strengths of [unclear], not whether anyone has a computer system that does or does not represent a particular channel of influence.[61]

However, not all the economists I spoke to lay within this area of consensus. On the one hand, some economists question the demand-side emphasis of the mainstream IS-LM models:

Ormerod These macro models, their problem is that the theory which lies behind them, I think, is basically wrong. The economy isn't driven by demand, its more supply side driven; its driven by productivity and level of profitability and investment. Its the whole approach that's wrong.[62]

On the other, there are economists who believe that the problem lies with the way in which IS-LM models treat the supply side of the economy:

Godley Straightforward textbooks would have IS-LM plus a supply side. I mean IS-LM deals with aggregate demand, but there is also a thing called aggregate supply. I would accept the IS-LM part of it up to a point, but I wouldn't accept the aggregate supply part.[63]

In addition to these arguments about what the correct interpretation of basic economic theory another, possibly more contentious, issue in macro-economics is the extent to which explanations of macro-economic phenomena should be grounded in the optimising behaviour of individual economic agents; the so-called micro foundations. In macro-econometric modelling, this idea usually manifests itself as the Rational Expectations hypothesis, the proponents of which can be thought of as forming a sub-set of the micro-foundationalists.

Of the economists I interviewed, Patrick Minford is probably the most well known UK advocate of the Rational Expectations approach and believes it is one of the defining characteristics of the new consensus in macro-economics:

Minford All these models have IS-LM curves and a Phillips curve - you have 3 major relationships. In an open economy context its a little bit more complicated but its essentially an adaptation of that closed economy framework, and the disputes have been on the degree of rational expectations. But that in a sense, in itself shows you how far economists converge.[64]

Minford [in over 10 years] there's been no big challenge to this what you describe, this IS-LM, Phillips curve plus rational expectations, sort of new classical /

new Keynesian framework, which they all more or less operate in to differing degrees[65]

On the other hand, of the two mainstream economists I spoke to, one had stopped using rational expectations altogether[66], and the other was keen to differentiate himself from the full-blooded rational expectations methodology of the Liverpool group.

Britton On the question you raised about RE, in 1985 we introduced RE in a very different way to the way in which Patrick Minford and the Liverpool model had them. What we were doing actually was taking account of output expectations in order to get a *marginally*, I mean its not a vast difference, to try and get a marginally better explanation of employment behaviour, stockbuilding and one or two other things. In other words, decisions about volumes. It was very different to the sense in which RE was being discussed, particularly in the Liverpool model, where they were assuming that the world corresponded to a very simple model in which events were totally transparent.[67]

Thus, despite Professor Minford's claim that Rational Expectations is a defining characteristic of the consensus in macro-economics, the mainstream econometric modellers appear distinctly lukewarm about the idea. The other modellers were more forthright in their rejections of the need to ground macro-econometric models in micro-economic theories:

Ormerod I think that, on a different point, though its connected, the aim of building up macro relationships from individual maximising behaviour is fundamentally flawed. Because of feedback in the system, peoples' behaviour is altered by observing other peoples' behaviour, and the behaviour of the macro economy may be quite different from the behaviour of an hypothesised individual on a desert island. It simply cannot be aggregated from individual behaviour.[68]

Godley Well ... when people speak of micro-foundations they tend to mean by that a very special thing, which is its all deducible in terms of the optimising behaviour of individual rational agents, and I don't accept that as an appropriate concept.[69]

From the preceding discussion we can see how economic modelling is perceived by its practitioners. I have shown how there are diverse opinions about all aspects of economic forecasting, ranging from the basic ontological conception of the economy

(changing or stable) to the appropriate theoretical tools to use in understanding it. In part this heterogeneity persists because of the ambiguity of econometric analysis which was highlighted in Chapter 2. The basic conclusion is that econometric testing does not discriminate between economic theories.

However, econometrics is not the only way in which an economic theory can be tested. Indeed there is a strong tradition in economics (usually attributed to Friedman's influential essay[70]) of testing theories by the accuracy of the predictions which they make about the world. If regression analysis is, for various reasons, unable to discriminate between economic theories, can forecasts be interpreted as testable hypotheses about the economy and forecasting performance used to evaluate econometric models?

Falsification and Crucial Experiments

In this section I examine how ideas of falsification enter into economic forecasting and show how the economists are able to maintain their state of chronic disagreement whilst simultaneously upholding their scientific ideals. In particular, I try to answer the question what persuades economists to adopt one specification rather than another or what does it take to falsify a model or an equation? The discussion focuses on the extent to which economic forecasts are regarded as crucial experiments, and the *ad hoc* strategies used to defend these apparently rejected hypotheses.

Falsification

The 'forecasts-as-falsification-tests' position receives its clearest expression in the following quote:

> *Minford* There are two main ways in which the profession changes its view. One is if a theory makes palpable nonsense, they jettison it, or if another theory comes and makes sense they may accept it ... The other things that prove irresistible are big forecast errors.[71]

The reason forecast tests are important is that, as we have noted, conventional econometric tests are not sufficiently restrictive to weed out all the 'wrong' models. Thus, in a nutshell, 'it is [not] possible for econometrics to actually test economic theories over short periods of time, or on actual data.'[72] One economist summed up the problem as follows

Ormerod People can design models which may have quite important differences in their policy conclusions, but all of them are quite capable of producing models which satisfy these design criteria [i.e. econometric tests], and not one of which is unequivocally superior.[73]

Similarly:

Minford One is reminded that there are quite a lot of models which are observationally equivalent in terms of their variances, but which nevertheless are quite different representations of the world.[74]

Perhaps unsurprisingly, not everyone agrees with this assessment

Wallis What one does then try to do, from an econometric point of view, is to ask how good the model was, or how good the model is, and we have ways of doing that, and I would be much happier taking those things, those analyses, to be the tests of the theory, rather than the failure of the published forecasts. I think the evidence in the economics community is that, by and large, they don't take forecast failures as crucial experiments.[75]

There are also more sophisticated arguments in which the stochastic nature of econometrics is stressed. In this argument, forecasting mistakes are to be expected and are nothing more than bad luck as there is always a chance (typically 1 in 20) that the evidence will not conform to the theory.

Britton [Forecasts] are all probability statements. All econometrics is based on the idea of probability criteria, that you are going to say that, you know, the likelihood of this result not being consistent with theory is below 5% or something, and you have to cut off at some point. You don't get the one definitive experiment which shows you that, you know, the speed of light is the same in all directions[76].

Alternatively, the idea of a quantitative falsification can be rejected as inappropriate.

Godley I don't really agree with that way of looking at it. If you're going to look at the forecasting operation as being GDP going up 3% and that kind of thing, I wouldn't accept that. I wouldn't accept that as I think that all the people who do that sort of forecasting, in that way, are really doing a stupid thing... The concept of judging a forecast in terms of a number and its relationship to another number which is later published [is wrong]. A forecast ought to convey the whole character of the forthcoming period; it shouldn't be thought of as being an entirely quantitative thing.[77]

The argument thus turns on what is the appropriate test for an econometric model. In the remainder of this section I will demonstrate that whichever option you choose, econometrics or forecast mistakes, the conclusions are always ambiguous because *ad hoc* strategies are always available. To give an example and set the scene, consider the following, from a proponent of the UK's membership of the European Exchange Rate Mechanism (the ERM):

> *Currie* We were strong exponents of membership of the ERM, and we thought that it was sustainable. It turned out not to be sustainable. Well that may be because we chose the wrong rate, it could be that fixed exchange rates are a disaster as Patrick Minford would argue, or it could be because the Bank of England made a lot of tactical errors, and its easy to argue that. Its very hard to know.[78]

Ad hoc Responses

Taking first econometric testing of models it is, as has already been noted, extremely difficult to get a 'clean kill' on a theory. Some of the reasons given for this by economists are as follows:

> *Minford* I think most econometrics is rubbish frankly, there's an awful lot of sheer bullshit published in the journals where people purport to have tested something. But its balony, because there's 5 other runs on the same data with slightly different specifications, with slightly different sample periods, which have either supported the [null] hypothesis or only marginally discounted it.[79]

> *Ormerod* Even within the same political framework, what is a relevant definition of wealth in a consumption function? There are many different ways of looking at it. I mean is it important for example to consider the impact of inflation on income or whatever?[80]

> *Britton* I think the reason why these statistical methods we use don't produce success more often is that we're always dealing with a sample which we know is too small. If you asked, you know, what size of sample would you like to have in order to estimate the relationship between imports and activity using quarterly observations, I'd say about 300 years would begin to be adequate, and we've actually got about 30.[81]

> *Britton* If you look at the theory which says that real interest rates should influence investment, its very difficult to find many studies which convincingly show

it does. This is not necessarily to say it has no effect, but just that for practical purposes this effect, which theory would emphasise, is actually rather difficult to see in the data. Of course, there are always reasons [and] the way in which the falsification of one hypothesis generates the next one makes it a little more complicated - its not just the real rate of interest, its the post-tax real rate of interest, and so on.[82]

For these, and many other, reasons econometric evaluation is always open to re-interpretation at the hands of a determined sceptic. This is not to say that there is necessarily anything wrong with this. Indeed, that scientists use *ad hoc* strategies to defend their beliefs is common currency to many philosophies of science and should surprise no-one. The point is simply that, taken alone, econometric evidence is unlikely to persuade anyone to hold or reject a particular theory.

What then of the 'big forecast mistake' test - is this anymore forceful in practice? Certainly the rhetoric of the 'big forecast mistake' has straightforward down-to-earth appeal, which can contrasted with the technical and evasive qualifications of formal econometrics. The argument is that because econometric tests can be very sensitive to the sample period used, and are known to be so, economists are relatively unimpressed by them. In contrast, a model which can reliably predict the future accurately is a model which commands respect.

Minford The empirical tests that are favoured in practice by the economics community are very robust ones, just like the City and Business actually. [When] Joe Economist evaluates models and he doesn't really look at fancy estimation or any of this.[83]

'Big Forecast Mistakes'

An obvious pre-requisite for applying the 'big forecast mistake' test is to be able to identify a forecast which was clearly wrong. Although this sounds (as it is no doubt meant to) quite simple it turns out to be rather difficult in practice. To begin with, some forecast mistakes are judged to be more important than others. For example, when asked why the Liverpool model had not been falsified when it completely failed to predict the 1980-81 recession, Patrick Minford replied

Minford Sure, sure, but I'm not talking about, I'm talking about the big errors, the big errors. Now of course, we didn't call the recession we saw in 1980 for example. We forecast that the Thatcher policies would deliver a mild recession, a growth recession, but a tremendous drop in inflation, but other

people were saying there'd be something more of a recession but no drop in inflation.[84]

Thus, the status of the Liverpool forecast as a 'big mistake' is open to question. Needless to say there are those who would argue that predicting that output would remain constant when, in fact, it contracted by nearly 3% must surely count as a major forecasting disaster.[85]

In is interesting to note the other side of this particular coin. Just like 'wrongness', so too the 'rightness' of a forecast has also to be established and, as such, can also be disputed. Thus, it is possible a numerically correct forecast to be discounted because it is for the 'wrong' reasons:

Minford Cambridge... got unemployment right because they were very bad on output of course. They were too optimistic on their unemployment-output relationships, so they got unemployment right but they got the mix if you like, rather than unemployment, wrong, like everybody else. So I don't think people were terribly impressed by the Cambridge thing, they just felt they were that they were just far too pessimistic on demand and output and felt that they got unemployment right by mistake.[86]

Again this perception is not shared by everybody

Godley Cambridge Economic Policy Group was very good.

Evans For unemployment, but Patrick Minford's group was very good for inflation. There's an article by Ken Holden

Godley Our forecast for inflation was very good

Evans But if you compare the errors, who did the best ones, then apparently Liverpool are the best for inflation

Godley I don't believe its true.[87]

In addition to just being the lucky result of mistakes elsewhere in the econometric model, a correct forecast can also be produced simply by chance, and thus confer no credibility to either the model or modeller. For example, Tim Congdon is generally credited (at least by fellow monetarists) with calling the inflation of 1989 and 1990 very accurately. However, non-monetarists are less convinced:

Currie You need to go back with Tim. I mean Tim forecast the inflation of 88-89, but he also forecast it back earlier. I mean he was forecasting that for some

considerable time, [and] if you go look at the growth of broad money you see why, because it was growing very fast for some time without causing any explosion. So the question you have to ask is that a forecast, is that a forecasting triumph, or not?[88]

Even in the case of an acknowledged forecast failure, we can identify a number of reasons why a discrepancy between the forecast and the outturn will not necessarily lead to the rejection of the model. These issues have been discussed at length elsewhere[89] and I offer only a brief summary of the conventional decomposition here. Firstly, the data used to estimate the model may subsequently be revised thus changing the definition of the recent past and hence the parameters of the model. Secondly, in order to use the model to forecast, the future values of all variables not determined by the model (i.e. the exogenous variables) must be input directly. If the projections made for these exogenous variable are not fulfilled then the forecast may turn out to be wrong but this does not imply that the model itself was at fault. Indeed it remains a possibility that had the exogenous conditions been met then the forecast would have been fulfilled[90]. Thirdly, some of the judgmental adjustments made might, with the benefit of hindsight, be regarded as inappropriate.[91] It is only after all these possibilities have been exhausted that the mis-specification of the model becomes an issue. Thus, even if the modellers agree that there has been a forecast mistake, the next step need not be a rejection of anything as *ad hoc* rescues are always possible:

Currie It may be that we had overlooked something ... It doesn't necessarily force us to reconsider our ideas. On the other hand it might, if it goes far enough, and then it depends very much on how strong the priors are on one's judgement about these sorts of thing.[92]

Even if it eventually has to be conceded that the model is mis-specified this need not reflect too badly on the modelling team. In discussing the National Institutes failure to predict both the boom and recession phases of the last economic cycle in the UK Andrew Britton explained

Britton [Its possible that the model was always wrong] but in fact that wouldn't have explained why forecasting performance had deteriorated. The fact that virtually all the models, all the sort of formal fully developed models failed to predict, suggests that it was not that our model was particularly bad, but that the underlying economy had changed[93]

In other words, the problem is that the economy no longer matches the model - it was the economy which was wrong. This observation brings as back to the beginning of this section where is was noted that change in the economy can render econometric models obsolete as representations of the economy.

Conclusion

In this chapter I have shown how the fluid and open nature of macro-modelling is maintained. Because econometric testing is chronically ambiguous no one model, theory or specification can be shown to be unambiguously superior to the rest. In addition, forecast mistakes are similarly ambiguous and, even when acknowledged, do not force any particular course of action on the modelling team. What this means is that economic modellers are trapped within an experimenters' regress from which they are unable to break free. Because they are unable to agree on what the correct outcome should be, they can never know when an experiment has been carried out competently, thus establishing the veracity of the original knowledge claim.

In this situation, which might be likened to a constant controversy, economists must resort to other, non-econometric methods to buttress their arguments. The question raised by this is as follows: if the 'formal' procedures are indeterminate what actually is it that persuades economists to adopt one course of action rather than another. Are there other sorts of reasons for believing an economic forecaster, and if so what are they?

> *Minford* I think the econometrics is important, [but] it's only one fairly moderate constituent in the whole process of fielding a competitive modelling effort. You have to have a theory that is at once decent, consistent and respectable; persuades you and others. It also has to be sufficiently simple to be implementable. It has to be reasonably stable over time so its recognisable. And then it has to pass these much more informal tests of can you tell stories about what's going on. Can you, if you have made a forecasting error, can you say why? What is it that you got wrong? Obviously its got to avoid major forecasting catastrophes, that's absolutely crucial.[94]

It seems therefore that, as McCloskey has argued[95], the supposedly scientific part of econometric modelling, the econometric testing, does not count for much amongst economists. It is McCloskey's belief that economists are ultimately persuaded not by statistics, but by a well-reasoned argument, clearly and lucidly expressed. From my own interviews it seems clear that this view is shared by others:

Evans What actually does resolve disputes in economics

Godley Nothing!

Evans They just go on. Well, they certainly seem to

Godley Successful rhetoric is what resolves issues.[96]

Summary

This chapter began by examining the statistical uncertainty which is associated with an econometric model and its forecasts. I argued that if the 'error bars' which bound an econometric forecast were to be formally estimated they would enclose a wide range of economic outturns, and that this range might be large enough to undermine the credibility of the whole enterprise. It was then shown how this 'uncertainty' is reduced and managed by the economists, principally through re-defining the question into one about past errors and limiting the dissemination of information.

The second theme developed in this chapter has been the 'interpretative flexibility' of macro-econometric modelling. In this chapter I have used the juxtaposition of interview quotes to demonstrate the openness and variety of discourses within macro-modelling and to illustrate how all economic data are capable of sustaining alternative hypotheses. I have tried throughout to show how each claim has its counter-claim, how each position in every argument is capable of supporting an alternative hypothesis. Thus, econometric models are seen by some as objective and replicable, to others they are inextricably bound up with the modeller. As with other branches of science, tests are subsequently seen to be inadequate and falsified hypotheses are rescued. However, and this is where econometric modelling differs from other branches of science, the situation in which many different models are equally compatible with the evidence and no one specification is unambiguously superior to another seems to have become an institutionalised way of life. Indeed, the main conclusion which can be drawn from this chapter is that is difficult to see how the chronic controversy in economics can be resolved.

Notes

[1] The phrase is Kip Thorne's. See Collins, H.M. (1985) *Changing Order: Induction and Replication in Scientific Practice*. Sage: London.

[2] Collins, H.M., op cit. note 1.

[3] The 'plane of best fit' can be visualised as follows:

> [Imagine you are] standing in a corner and facing the centre of a room. Take as the y-axis the line running from the floor to the ceiling that is created by the joining of the two walls at your back. Now take the axis along which the X_1 data are to be plotted as the line joining the floor to the wall on your left, and take as the X_2 axis the line joining the floor and the wall to your right. The position of the individual data points (Y_i, X_{1i}, X_{2i}) in the room are then determined by finding where X_{1i} is along the left wall, where X_{2i} is along the right wall, finding where lines perpendicular to the respective axis intersect on the floor (out in the room somewhere), and then rising vertically by the amount of Y_i. BY doing this for each of the observed data points the room then contains all the observed data points at various spots and at various heights. The best fitting plane to these data points can then be thought of as a platform suspended from the ceiling that is tilted in two directions to fit the observed data.

source: Wallace, T. Dudley and Silver, J. Lew (1988) *Econometrics: An Introduction* Massachusetts, USA: Addison Wesley Publishing. p. 92, note 3.

[4] This set of error terms has a normal distribution, with zero mean and a standard deviation which can be calculated from the observed differences between the estimated and actual values of the dependent variable.

[5] However some modelling teams are attempting to re-estimate their models using full information techniques.

[6] Interview, Andrew Britton, 29 April 1993, p. 27.

[7] Interview, Ken Wallis, 21 March 1993, p. 5.

[8] The paper Currie is referring to is David Hendry 'The Role of Prediction in Evaluating Econometric Models' in John Mason, P. Mathias and J. H. Westcott (eds.) *Predictability in Science and Society: A Joint Symposium of the Royal Society and The British Academy Held on 20 and 21 March, 1986.* (London: Royal Society and British Academy, 1986); first published in 'Proceedings of the Royal Society of London, Series "A" - Mathematical an Physical Sciences', Vol. 407 (1986), Part 1832, 25-34.

[9] Interview, David Currie, 13 May 1993, p. 14.

[10] See e.g. Wallis, K. F., & Whitely, J. (1991). 'Sources of Error in Forecasts and Expectations: UK Economic Models, 1984-8'. *Journal of Forecasting*, **10**, pp. 231-253; Turner, D. S. (1990). 'The Role of Judgement in Economic Forecasting'. *Journal of Forecasting*, Vol. 9, pp. 315-45. For a criticism of the routine use of judgemental adjustments see: Wren-Lewis, S. (1992). *Macroeconomic Theory and UK Macroeconomic Models: Another Failed Partnership?* (Discussion paper 9), International Centre for Macroeconomcic Modelling, University of Strathclyde;

[11] Interview, Paul Ormerod, 16 March 1993, p. 2.

[12] Interview, Patrick Minford, 31 March 1993, pp. 2-4

[13] See e.g. Kenneth F. Wallis (ed.), M. J. Andrews, P. G. Fisher J. D. Whitley, (1984) *Models of the UK Economy: A Review by the ESRC Macromodelling Bureau.* Oxford: Oxford University Press.

[14] Andrew Britton, op cit. note 6, p. 27.

[15] Ken Wallis, op cit. note 7, p. 5.

[16] One of the economists I spoke to however told me that this strategy had been attempted by the National Institute and that they had been ridiculed for it.

[17] In addition, the relative weight assigned to the model and modeller are unclear. This poses problems when a person leaves one forecasting group and joins another: should his or her record be transferred to the new group?

[18] Interestingly, the National Institute, in its *Economic Review*, did, during the 1980s head each forecast table with the disclaimer: 'the forecast figures are not intended to be anymore precise than the general statements in the text'. (see e.g. *National Institute Economic Review* (1988) No. 123, February 1988, p. 7, Table 1)

[19] I am reminded of an profile of the BBC economist Peter Jay in the *Independent on Sunday*. In the article it told of how Jay, when working as a writer for the Times was reprimanded by his editor for writing columns which were

unintelligible to the lay person. Jay's response was that he was not writing for the general public, just that portion of it which made economic policy.

[20] Patrick Minford, op cit. note 12. p. 19

[21] Andrew Britton, op cit. note 6, p. 35.

[22] Patrick Minford, op cit. note 12, p. 19.

[23] Perhaps mirroring the science-technology division in the natural sciences.

[24] David Currie, op cit. note 9, p. 15.

[25] There is however a strong similarity with climate modelling and forecasting, particularly with regard to climate change where 'flux adjustments' seem to function in a very similar way to the 'residual adjustments' used by economists. For an interesting account of the ways in which climate scientists manage the tension between 'uncertainty' and 'authority' see: Shackley, S. and Wynne, B. (1996) 'Representing Uncertainty in Global Climate Change Science and Policy: Boundary Ordering Devices and Authority' *Science Technology and Human Values*, **21(3)**, pp. 275-302.

[26] This difference is very important in economics. When estimating equations for an econometric model, the variables are usually transformed into logarithms before the regression analysis begins. This means that the coefficients will correspond to that elasticities on the variables, and elasticities are important technical concepts in economic analysis. An elasticity of 1 or more means that the dependent variable is highly sensitive to changes in the independent variable. If the coefficient is leas than one this implies that the relationship is inelastic, i.e. that the dependent variable is relatively insensitive to changes in the independent. Thus the difference of 0.4 is not simply a matter of magnitude but of meaning.

[27] If the error terms of a regression equation are correlated and the estimation process does not take this into account then the coefficients cannot be efficient estimators as all the available information, in particular the fact that the error terms are not random but systematically related, has not been used.

[28] In time series econometrics data which dates from periods before the one in question is called lagged data, and changing the lag structure in an equation means changing the time allowed for a change in the independent variable to affect the dependent variable. For example, instead of saying that investment is a function of interest rates in the last quarter (i.e. interest rates lagged one quarter), we might say that investment is determined by the level of interest rates four quarters ago (i.e. lagged four quarters). Alternatively, both lagged values might be important.

[29] Lucas, Robert E. Jnr (1976) 'Econometric Policy Evaluation: A Critique' in Karl Brunner and Allan H. Meltzer (eds.) *The Phillips Curve and Labour Markets*, Vol. 1 of the Carnegie-Rochester Conferences on Public Policy, a supplementary series to the *Journal of Monetary Economics* (North-Holland, Amsterdam), pp. 19-46

[30] Although the Lucas Critique relates to a particular type of structural change (i.e. endogenous policy change), it is possible to extend the same idea to structural change more generally.

[31] Ken Wallis, op cit. note 12, p. 26.

[32] Interview, Wynne Godley, 5 April 1993, p. 1.

[33] David Currie, op cit. note 9, p. 7.

[34] Andrew Britton, op cit. note 6, pp. 4-5

[35] The distinction between strong and weak economics is also made by Ashmore, Mulkay and Pinch in their study of health economics. Ashmore, M., Mulkay, M. and Pinch, T. (1989) *Health and Efficiency: A Sociology of Health Economics*. Milton Keynes: Open University Press.

[36] Paul Ormerod, op cit. note 11, pp. 4 and 16.

[37] Patrick Minford, op cit. note 12, p. 14.

[38] Patrick Minford, op cit. note 12, p. 5.

[39] Patrick Minford, op cit. note 12, p. 12.

[40] Andrew Britton, op cit. note 6, pp. 3-4.

[41] Ken Wallis, op cit. note 7, p. 9.

[42] The adoption of a scientific methodology also requires the economists to conform to particular view of science in other areas of their work. For example during the period in which these interviews took place one of the Panel of Independent Forecasters published a series of open letters in which he heavily criticised the other panel members. These letters provoked quite a lot of reaction at the time, apparently offending several panel members. One of the economists I interviewed said of the letters:

Wallis You can disagree pleasantly. You don't have to resort to personal attack simply because you take a different point of view ... I don't think physical scientists go for each others throat in quite that way. (op cit. note 7, p. 20)

On the other hand, other respondents felt that the letters were a legitimate tactic in the publicity game, remarking that their author was in for a tough time at the next meeting!

[43] Andrew Britton, op cit. note 6, p. 4.

[44] see Collins op cit. note 1.

[45] What I have termed the strong in economics corresponds to what I believe to be the received view of economics and also the perception of the lay population. Economics is a normal science solving puzzles and accumulating knowledge.

[46] This methodology does not have the unqualified support of all economists however. One of those I interviewed rejected regression outright saying:

Ormerod It was clear to me some years ago that macro models based on conventional economic theory and then parameterised by econometrics couldn't do what they claimed to be able to do, which was they claimed to be able to do short-run forecasting. (p. 2).

Another was critical not so much of regression analysis *per se* but rather of the idea that by expanding the regression equation to include more explanatory variables better forecasts would be produced

Godley I think that the whole, the conventional, or the conventional aspiration in regard to macro economic modelling is wrong. There's a fantasy that you're going to introduce more and more complexity and more and more realistic features and apply better and better

Evans Statistical techniques, and use bigger and bigger computers

Godley And eventually it will work, and it will give you the answer. You see its all been a great failure, all of that, and forecasting isn't done better, it makes no progress, as a result (p. 7).

[47] More precisely, one can reject, at a given confidence level, the null hypothesis that there is no relationship between the variables.

[48] David Currie, op cit. note 9, p. 4.

[49] See e.g. Turner, D.S. (1990) 'The Role of Judgement in Economic Forecasting'. *Journal of Forecasting*, Vol. 9, pp. 315-45

[50] It is also possible to argue that the frequency with forecasts are published, together with their easy availability, make questionable the assertion that the different modelling groups are actually independent of one another.

[51] The fact that this is the same economist who earlier praised the virtues of the formal procedures, and dismissed judgement as soothsaying should not trouble us too much. Research in discourse analysis should have led us to expect such variability in accounts (see e.g. Mulkay, M. and Gilbert, N. (1984) *Opening Pandora's Box: A sociological analysis of scientists' discourse.* Cambridge University Press: Cambridge.). The quote is from Britton, op cit. note 6, p. 35.

[52] Andrew Britton, op cit. note 9, p. 3.

[53] Wynne Godley, op cit. note 32, p. 5.

[54] How would one replicate Wynne Godley's economic forecasts?

[55] This raises the important question of decisions of similarity and difference. For example, the effect of a decision to levy a tax on previously untaxed goods or services could probably be predicted so long as the price of those goods and services had varied by a the same order of magnitude in the period over which the equation had been estimated. In these circumstances, the tax is the 'same' as any other price rise and can, *ceteris paribus*, be expected to have the same effect. In circumstances where the price has never changed pure econometrics has very little to say, and the only option left is for the modeller to impose a residual adjustment.

[56] Ken Wallis, op cit. note 7, p. 2.

[57] Patrick Minford, op cit. note 12, p. 16.

[58] Paul Ormerod, op cit. note 11, p. 8, emphasis added.

[59] Wynne Godley, op cit. note 32, p. 15.

[60] Patrick Minford, op cit. note 12, p. 25.

[61] Ken Wallis, op cit. note 7, p. 18.

[62] Paul Ormerod, op cit. note 11, pp. 6-7.

[63] Wynne Godley, op cit. note 32, p. 2.

[64] Patrick Minford, op cit. note 12, p. 24.

[65] The quotation continues:

> *Minford* ... except for Tim Congdon who uses a pre-deluvian IS-LM Phillips curve set-up with some sort of notional adaptive expectations, but I think that's because he is relatively immune to what is going on in academic, you know he's not terribly interested in, he's not an academic, so he doesn't really relate to the profession in the way that the rest of us do.
>
> Wynne Godley's the same actually. Wynne Godley doesn't really have any expectations in his model at all. Its got these deficits and surpluses, and so on. I think that both Wynne and Tim are really, in their different ways, fairly uninterested in what most academics are up to (p. 26).

The source is Patrick Minford, op cit. note 12, p. 26

[66] Strictly speaking rational expectation were introduced to the LBS model by Alan Budd, now at the Treasury. David Currie was appointed director of the LBS from 1988, and rational expectations were not dropped until 1990 (approx.).

[67] Andrew Britton, op cit. note 6, pp. 6-7.

[68] Paul Ormerod, op cit. note 11, p. 14.

[69] Wynne Godley, op cit. note 32, p. 7.

[70] Milton Friedman 'The Methodology of Positive Economics' in M. Friedman, "Essays in Positive Economics' (Chicago: Chicago University Press, 1953), pp. 3-43

[71] Patrick Minford, op cit. note 12, pp. 7-8.

[72] Patrick Minford, op cit. note 12, p. 5.

[73] Paul Ormerod, op cit. note 11, p. 12.

[74] Patrick Minford, op cit. note 12, p. 30.

[75] Ken Wallis, op cit. note 7, p. 14.

[76] It is interesting to note the idea of science used here. For a sociological analysis of the Michelson Morley experiments see: Collins, H.M. and Pinch, T.J. (1993) *The Golem: What Everyone Needs to Know About Science.* Cambridge University Press: Cambridge. The source is Britton, op cit. note 6, p. 17.

[77] Wynne Godley, op cit. note 32, p. 3.

[78] David Currie, op cit. note 9, p. 4.

[79] Patrick Minford, op cit. note 12, p. 6.

[80] Paul Ormerod, op cit. note 11, p. 12.

[81] Andrew Britton, op cit. note 6, p. 25.

[82] Andrew Britton, op cit. note 6, p. 18.

[83] Patrick Minford, op cit. note 12, p. 31.

[84] Patrick Minford, op cit. note 12, p. 6.

[85] It is interesting to note the prominence given to inflation during the 1980s. To the extent that the government, a prime user of forecasts, and the wider society chooses to prioritise inflation then those models which predict inflation most accurately will come to be seen as the 'best'.

[86] Patrick Minford, op cit. note 12, p. 11.

[87] Wynne Godley, op cit. note 32, p. 9.

[88] David Currie, op cit. note 9, p. 11.

[89] See e.g. Wallis *et al*, op cit. note 13.

[90] It should be noted however, that if exogenous projections are subsequently replaced by the actual values forecasts are not generally improved. See e.g. Wallis *et .al*, op cit. note 13.

[91] In the Evans (1993) I argued that the separation between the exogenous adjustments and judgmental adjustments was not a clear one (Evans, R.J. (1993) *Economic Modelling: A Sociological Perspective.* Unpublished dissertation for MSc in Social Research, University of Bath). In the course of this project I discovered that this concern is also shared by some economists

> *Wallis* Right, that was the way we did it. Its arguable whether the actual comparison is a fair one, whether there isn't some interaction, well undoubtedly there will be some interaction between the exogenous variable projection that people are using and the adjustments they are making, because what they are actually adjusting, what they are working with, is a provisional forecast based on those projections (p. 14).

[92] David Currie, op cit. note 9, p. 3.

[93] Andrew Britton, op cit. note 6, p. 11.

[94] Patrick Minford, op cit. note 12, pp. 22-23.

[95] McCloskey, D.N. (1986) *The Rhetoric of Economics.* Brighton: Wheatsheaf Books

[96] Wynne Godley, op cit. note 32, p. 17.

Chapter 5

This chapter is the first of 3 which examine the forecasts produced by the Seven Wise Men during 1993. The aim is to look at the reasons given by each of the forecasters when they were asked to explain why they had forecast the way they did and also to explain where they thought the others were most likely to be caught out. The aim therefore is to show how each forecast is the complex balancing of judgement, beliefs and hunches. By imposing these beliefs upon the models, the econometric forecasts can be finessed into something which the economic forecaster is able to defend. This chapter thus shows how the very different forecasts for 1993 were justified by the proponents. The remaining chapters show how these interpretations are maintained and modified as the year progresses and new data and evidence become available.

Forecasting 1993

February 1993

In February 1993, the forecasts produced by the Treasury's Panel of Independent Advisers ranged from reasonably optimistic to extremely bleak. A summary of the forecasts for the major economic indicators is given in Table 1.

Table 1: Summary of Panel of Forecasters' Short-Term Forecasts[1]

	GDP	Domestic Demand	Net Trade[2]	Unemployment[3]	RPIX
Britton	2.0	1.8	0.1	3.2	4.6
Congdon	1.1	0.5	0.7	3.3	3.5
Currie	1.4	1.6	-0.1	3.2	4.0
Davies	1.5	1.4	0.1	3.2	3.6
Godley	0.5	0.2	0.3	3.4	4.8
Minford	0.2	-1.0	1.3	3.1	3.8
Sentance	0.7	0.3	0.3	3.1	3.1
Average	**1.1**	**0.7**	**0.4**	**3.2**	**3.9**

[1] *Percentage changes on a year earlier unless otherwise stated.*
[2] *Contribution to GDP growth, per cent.*
[3] *Millions.*

Part of the variation in the forecasts made in February 1993, is due to the considerable uncertainty which remained about the economic data for 1992. In other words, some of the differences in forecasts for 1993 can be attributed to differences in the 'backcasts' for 1992. In fact, as we shall see later, data revisions in mid 1993 had quite a significant effect on several forecasters. However, just to give an example of the uncertainty about

what had come before consider the following. In February 1993, figures for domestic demand in 1992 ranged from -0.5 (Godley) to 0.2 (Congdon); for Net Trade, from -1.4 (Congdon) to -0.4 (Godley). In other words, although there was a general agreement that GDP had contracted by about 0.8 per cent in 1992, there was much less consensus about where the contraction had occurred.

Forecasts for GDP Growth and its components

GDP growth is one of the most important indicators of economic health and well being and its forecasts are therefore very significant. Amongst the Panel there was a consensus that the economy was now more likely to resume some sort of economic growth as a result of the exit from the Exchange Rate Mechanism (ERM). However, there remained a range of views regarding the extent of this recovery:

> The substantial relaxation of monetary policy since sterling's suspension from the ERM has greatly improved the prospects of recovery in 1993. However, there is considerable uncertainty and disagreement about the likely strength of output. The average of our forecasts is for GDP to rise by 1% this year. But the range is from 0.25 to 2 per cent, with Minford and Godley expecting little, if any, recovery.[1]

In fact, a closer examination of the individual submissions reveals that the economists differ even more profoundly on other parts of the forecasts. Thus, the consensus view (that GDP growth in 1993 will at least be positive and that the recession is over) actually comes from quite different views about where the growth is going to come from. In particular, the relative contributions of domestic demand and net trade to overall economic growth vary considerably. In the case of domestic demand, the difference is not just one of magnitude but of *direction*:

> The average of our forecasts for **domestic demand** is a rise of 0.75 per cent in 1993 but the range stretches from -1 to [+] 1.75 per cent. We agree that consumer spending is likely to be restrained by balance sheet problems (for example, the large overhang of debt), the weakness of the housing market, rising unemployment and a squeeze on real incomes caused by rising import prices: variations between our forecasts largely reflect different *judgements* about the extent to which these factors will hold back spending. There is less consensus about the prospects for business spending and government consumption. However, most of us agree that government will make very

little contribution to growth in 1993 and 1994, and only Britton and Currie expect any contribution to growth from fixed investment in 1993.[2]

The way in which these forecasts differ and develop as 1993 progresses is shown in the following tables. Table 2 and Table 3 (overleaf) show the forecasts for a selection of key economic variables which were produced by the Panel members at each of their meetings. Of particular interest in this and later chapters will be the differences within columns (i.e. the differences between forecasters). Although the differences within rows (i.e. the differences between forecasts made at different times) are also interesting, the primary focus in this thesis is not how new information changes forecasts, but on the variety of interpretations which can be put on essentially the same data.

From Table 2 we can see that, in February forecasts for GDP growth in that year ranged from 0.2% to 2.0%. In addition, and perhaps more interestingly, the kinds of growth being forecast also differed quite dramatically. For the three most optimistic forecasters (Britton, Davies and Currie), the main engine driving economic growth was to be Domestic Demand, with Net Trade either contributing little to the recovery or actually hindering growth. However, all three were concerned that the recovery would prove to be short lived because unemployment could not be reduced without an increase in demand so dramatic that deflationary policies would be required to contain inflation. Thus, to ensure the future sustainability of the recovery, new policies would need to be enacted in order to divert resources from consumption (public and private) and into investment.

The remaining 4 economists on the Panel offered rather different pictures. Sentance and Godley were essentially pessimistic in both the short and medium terms. Each forecast only minor increases in both Domestic Demand and Net Trade, and thus offered a future characterised by continued economic stagnation. Congdon's forecast was similar to Godley's and Sentance's view, in that recovery was more or less equally split between domestic and foreign markets. However, he was significantly more optimistic than either of these, believing that, providing the appropriate policies are followed, several years of above-trend growth, coupled with low inflation were likely.

Table 2: Forecasts for GDP, Domestic Demand and Net Trade made during 1993[1].

	February 1993			July 1993			October 1993			February 1994		
	GDP	*Domestic Demand*	Net Trade[2]	**GDP**	*Domestic Demand*	Net Trade[2]	**GDP**	*Domestic Demand*	Net Trade[2]	**GDP**	*Domestic Demand*	Net Trade[2]
Britton	**2.0**	*1.8*	0.1	**2.0**	*2.0*	0.0	**2.0**	*1.1*	0.9	**2.0**	*1.7*	0.3
Congdon	**1.1**	*0.5*	0.7	**1.5**	*0.9*	0.7	**1.8**	*1.1*	0.8	**2.0**	*1.6*	0.6
Currie	**1.4**	*1.6*	-0.1	**1.5**	*2.4*	-0.8	**1.6**	*1.1*	0.7	**2.0**	*1.6*	0.5
Davies	**1.5**	*1.4*	0.1	**1.7**	*2.1*	-0.5	**2.0**	*1.0*	1.1	**2.0**	*1.5*	0.5
Godley	**0.5**	*0.2*	0.3[3]	**1.2**	*1.3*	0.0[1]	**1.3**	*1.2*	-0.2[1]	**2.0**	*1.0*	0.8[1]
Minford	**0.2**	*-1.0*	1.3	**1.5**	*1.0*	0.6	**1.5**	*0.4*	1.1	**2.0**	*1.3*	0.6
Sentance	**0.7**	*0.3*	0.3	**1.6**	*1.3*	0.3	**1.7**	*1.1*	0.1	---	---	---
Average	**1.1**	*0.7*	0.4	**1.5**	*1.6*	0.0	**1.7**	*1.0*	0.8	2.0	*1.5*	0.5

[1] *Percentage changes on a year earlier unless otherwise stated*
[2] *Contribution to GDP growth, per cent.*
[3] *Non-Oil.*

Table 3: Forecasts for Inflation (RPIX)[1] and Public Sector Borrowing (PSBR)[2] made during 1993.

	February 1993		July 1993		October 1993		February 1994	
	RPIX	*PSBR*	**RPIX**	*PSBR*	**RPIX**	*PSBR*	**RPIX**	*PSBR*
Britton	**4.6**	*43.3*	**4.1**	*46.0*	**3.6**	*46.0*	**2.7**	*45.2*
Congdon	**3.5**	*44.0*	**2.9**	*41.0*	**2.8**	*45.0*	**2.7**	*45.0*
Currie	**4.0**	*49.5*	**3.5**	*52.0*	**3.2**	*49.1*	**2.7**	*47.0*
Davies	**3.6**	*52.0*	**3.0**	*48.0*	**3.4**	*49.0*	**2.7**	*48.0*
Godley	**4.8**	*55.0*	**3.7**	*42.0*	**3.5**	*42.0*	**2.7**	*42.0*
Minford	**3.8**	*40.0*	**2.5**	*42.0*	**2.9**	*43.3*	**2.7**	*42.3*
Sentance	**3.1**	*45.2*	**3.1**	*47.9*	**2.8**	*46.1*	---	---
Average	**3.9**	*47.0*	**3.3**	*42.2*	**3.2**	*45.8*	**2.7**	*44.9*

[1] *Percentage changes on a year earlier*
[2] *£ billion*

Finally, Minford believed that the domestic economic was very weak and that only a strong export performance would enable GDP to grow slightly in 1993. However, like Congdon, Minford is distinctly optimistic about the medium term.

From Table 3, we can see that a similar diversity of forecasts exists for inflation (RPIX) and the Public Sector Borrowing Requirement (PSBR). Here however, the interpretation is rather more complex. Britton forecasts that strong growth would lead to a relatively low PSBR, but that the pick up in demand would also be reflected in a rise in inflation. Currie, Davies and Sentance all told much the same story, although being less optimistic on demand, they foresaw less inflation and higher levels of Public Borrowing. In other words, the recovery was by no means assured and medium term policy had several difficult problems to address.

Minford and Congdon offered forecasts which, while numerically similar to those of Britton, Currie, Davies and Sentance, described a very different sort of recovery. In particular, both Minford and Congdon believed that the excess capacity created during the preceding recession meant that any inflationary effects of increasing demand would be muted. Thus, despite differing over the speed with which demand would pick up, they both saw inflation and the PSBR as being on downward trends over the medium term. Thus, for these two economists, the medium term outlook was rosy and presented few policy problems.

Finally, Godley's view was of an economy constrained by deep-rooted structural problems which mean that it is unable to sell goods in either domestic or world markets. Godley's view is that because economic growth will result in increased imports, and thus a worsening of the balance of payments, any pick up in demand will be have to be restricted, thereby increasing unemployment. The outlook for the medium term could therefore, according to Godley, be very bleak indeed unless very large changes in UK economic performance can be achieved.

These differences can be summarised by noting that they relate to three important relationships in the economy.

- GDP-Inflation
- Inflation-Wages
- Inflation-Interest Rates

The GDP-Inflation trade-off is perhaps the most important. The idea of a trade-off between GDP growth and inflation stems from the way in which growth in GDP, which is good, tends to raise prices, which is not so good. Conversely, policies which bring down inflation will also tend to slow down GDP growth. Thus, the goal for macroeconomic policy is to ensure that growth is sustained but that inflation remains low and the tendency for a good outcome on one measure to imply a poor performance on the other means that some sort of compromise is inevitable. However, the exact nature of the trade-off, i.e. how much can GDP grow before inflation takes off is unclear and this is a key difference between the Panel members.

The relationship between Inflation and Wages is rather different. Instead of there being a trade-off between wages and inflation, the metaphor most frequently used is that of a spiral. As inflation rises so wage claims also increase, this in turn increases prices and so on. The differences here stem from two main sources. One is the forecast for inflation. Clearly if inflation is low, then wage increases are also likely to be low. There is however an additional factor, and that is the trade-off between unemployment and wages. This is often referred to as the Phillips Curve. The idea is that if unemployment is high then wage inflation will be kept low by the strong competition for jobs. However, there are serious differences about the extent to which the unemployed can compete for employment and are thus able to keep wages low. This is a topic which was discussed in some detail by the Panel in their July meeting, and so I will postpone a detailed discussion of it until Chapter 6. However, it is worth noting that those on the Panel who believe that the unemployed do exert a strong influence on wages will be those who are most optimistic about the GDP-Inflation trade-off.

Finally there is the relationship between inflation (and hence output) and interest rates. The short term interest rate is the government's main instrument of monetary policy. Again, the differences here are not about the relationship itself, but about the likely path of inflation and the appropriate policy response. To caricature, there are those who see a poor GDP-Inflation trade-off, due in part to poor training programs for the long-term unemployed, and thus believe that inflation will rise soon and that interest rates will have to rise to control it. On the other hand, there are those who see flexible labour markets improving the GDP-Inflation trade-off to the extent that good outcomes are possible on both measures. This group therefore see no need for interest rates to rise and indeed some would even argue that the deflationary effect of high unemployment is

so strong that interest rates actually need to be cut to ensure that there is any significant growth in GDP at all during 1993.

The dilemma faced by the policy makers is thus at what level should interest rates be now to ensure that in 12-24 months time there will be continued economic growth with low inflation. Interest rates at too low a level imply accelerating inflation, while too high rates imply economic stagnation. It is thus a question of choosing which outcome one wishes to avoid the most:

> *Britton* It appears from the most recent cut in interest rates that the Government and the Bank of England are prepared to run a risk of adding to inflation in order to reinforce the recovery of output. If that is their priority, I sympathise with it[3].

In the remainder of this chapter I analyse in more detail how the differences outlined above are supported and how it is that each of the Panel members arrives at their forecasts. I begin by examining what they identify as being the key factors behind their forecast.

Theoretical Frameworks

In this section I set out the main factors identified by the Panel of Forecasters in their February 1993 submissions. Needless to say, different members of the Panel see different factors as being crucial and they differ on the reliability of several important indicators. Despite this, the views of the Panel members can be differentiated according to whether their analysis focuses on the devaluation or the effects of the recession which preceded it.

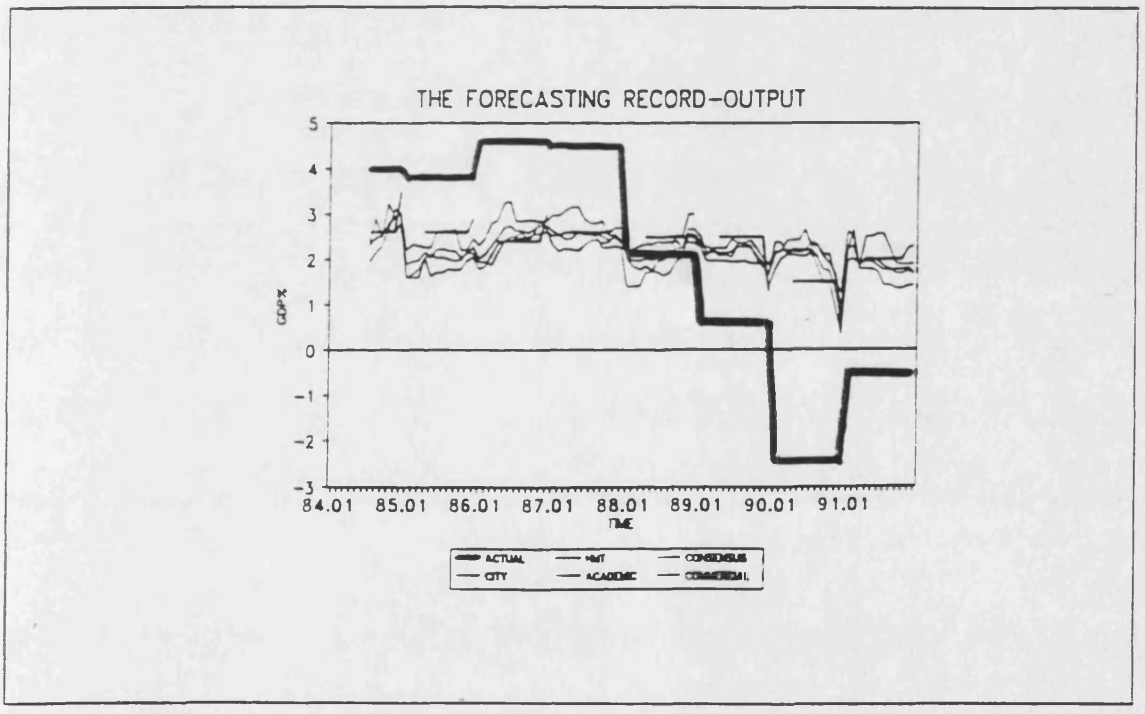
Taking first the 'Devaluationists', this group can be characterised as representing the mainstream consensus view, and its principal proponents are the National Institute and the London Business School. According to this view, the economy will recover during 1993, with economic growth resuming as a result of the fall in the sterling exchange rate. In addition, the low level of interest rates is expected to bring about a modest expansion in consumer spending. The analysis is summarised by David Currie as follows:

> *Currie* The past two years have seen the PSBR move from balance towards a deficit of around £50bn, interest rates cut from 15 per cent to 6 per cent, and the pound devalued by 15 per cent. This is a very large relaxation of policy: because the economy responds with a substantial delay, we have yet to see

its major impact on demand, output and inflation ... The 15 per cent devaluation provides an important impetus, boosting export volumes and holding back imports ... Low interest rates are expected to lead to growth in consumer spending, though slowly because of rising unemployment, uncertain employment prospects and continued risks about house prices[4].

As an aside, it is interesting to note that one effect of this appeal to precedent and evidence, is an implication that the analysis has a certain objectivity and robustness to it. This is important if one remembers the context in which the forecasts were being made. At the time (February 1993) macroeconomic forecasting was held in rather low esteem[5] as the result of a series of forecasts which had suggested that the recession would be much less severe than it actually was. The forecasting performance of the macromodelling industry is graphically illustrated in Figure 1.

Figure 1: One-Year Ahead Forecasts for GDP and Outcomes



Thus, in the following quotation, Andrew Britton's reference to the devaluation as the source of recovery distinguishes the National Institute's optimism from that of other forecasters who had, in the past, forecast recoveries which subsequently failed to materialise.

Britton [The] National Institute's forecasts for a recovery in the economy do not rest mainly on the indications of an improvement in confidence or in the levels of economic activity around the turn of the year. They rest rather on

> our analysis of the effects of the relaxation in monetary policy beginning from last September, especially the devaluation of sterling[6].

An interesting feature of the Devaluationists' analysis is that the persuasiveness of their forecasts now rests upon the response of the UK economy to previous devaluations. Thus Andrew Britton and David Currie both make reference to the response of the UK economy to the 1967 devaluation being a guide to what will happen in 1993 and beyond. In other words, the emphasis is shifted from the specific nature of the present case, to a more abstract discussion of 'devaluations in general'. However, the effect of this strategy is that the plausibility of the forecast is now linked to the plausibility of the idea that the economy is subject to regular and predictable sequences of events, i.e. that what happened in the past will happen again. As was shown in Chapter 4 it is by no means clear that this is the case.

Another economist for whom the devaluation is very significant is Wynne Godley. It has been a long-standing theme in Godley's economics that an economy can grow no faster than what is warranted by its performance in world trade. The seriousness of the situation, i.e. that it has now reached 'critical proportions', is made clear in the following quotation:

> *Godley* One structural problem is that the long term deterioration in Britain's competitive performance in world markets has now reached critical proportions. The trend of exports relative to import penetration has proved so relentlessly adverse that the current balance of payments is 2 per cent of GDP in deficit before any recovery at all has taken place[7].

For Godley, the long term deterioration in the competitiveness of UK exports is a crucial factor in explaining UK economic performance over the previous 20 years. Thus, to the extent that the devaluation has lowered the exchange rate and improved the UK's competitiveness, then it is a step in the right direction. However Godley also sees other problems in the UK economy which mean that the devaluation by itself will not be enough. In particular, it will not solve the long term problem of the UK's balance of trade deficit, which is in danger of, once again, imposing a constraint on UK economic growth. Godley is thus playing a much longer game than the mainstream modellers, and sums up the situation as follows:

> *Godley* Britain faces chronic depression and growing mass unemployment unless large scale structural changes now occur. Simple expansion of domestic

demand cannot provide even temporary alleviation. On the other hand, if the right policies are resolutely pursued, there is a reasonable chance that the necessary transformation can be brought about over a period of years. There has been a marked change in the stance of policy since September and I believe the Government is now essentially on the right track. It should on no account be panicked (for instance by accusations that it is "drifting") into changing course. Conceivably we have a unique opportunity for breaking out of the deeply entrenched and self-reinforcing process of decline which has afflicted the country for so long[8].

Interestingly, however, for Godley what is important about 1993 is not that there has been a devaluation like, say, 1967, but that the 'present conjuncture contains a number of well known features which, taken together, make it very different from the later stages of all previous recessions.'[9] In other words, it is the *uniqueness* of the situation which must be appreciated if it is to be properly understood and not its similarities to previous events. The principal factors which Godley identifies as making 1993 different are financial deregulation and the high debt levels associated with the credit boom of the 1980s. Thus, he writes:

Godley All earlier recoveries were assisted, if not initiated, by the relaxation of credit controls which caused a discontinuous increase in spending on durables. For instance, in the first quarter of 1983, following the removal of credit controls in mid-1982, consumption of durables was over 25 per cent higher than a year earlier. The same thing cannot happen this time as there are now no credit controls to relax. Moreover, while the flow of net credit to the household sector has fallen dramatically (by at least 10 per cent of disposable income) since 1988, it remained positive, at least until the middle of last year, so that total household debt is still nearly 100 per cent of disposable income - three times the proportion reached in 1974, at the peak of an earlier notorious credit boom[10].

In addition to the high levels of household debt, and the weak balance sheets and continued high saving which this implies, there has also been another change to the UK economy.

Godley Another structural problem arises because of the distortion in the composition of demand which has taken place during the last decade. Compared with the seventies, the share of domestic demand in GDP has risen by about six percentage points - the straightforward counterpart of the

move from surplus to deficit in the balance of trade. Within domestic demand, the share of GDP taken by fixed investment, after rising sharply in the late eighties, has fallen sharply during the recession, and is now back to the very low levels of 1981-2. By contrast, the share of personal consumption, which rose extremely fast during the boom to quite unprecedented levels in the late eighties, has hardly fallen back at all, although the credit boom was broken in 1990[11].

Again this change, which is related to the deregulation of credit, implies a change in the economy which must be recognised if economic developments in 1993 and beyond are to be properly understood and anticipated. In particular, it will be necessary, according to Godley, to reduce the share of personal consumption in GDP and to increase the share of investment and net exports if a sustainable balance is to be restored to economic activity. However, the problem is that, because household debts are so high, taxes to deter consumption are singularly inappropriate at this point because households are already extremely reluctant to spend. The time to raise taxes is at the point when the household sector has reduced its debt levels and its consumption has started to pick up. In the meantime, and this is where Godley differs significantly from the rest of the Panel, the PSBR may have to be increased in order to boost investment expenditure, particularly in the construction sector, in order to assure recovery. The increased PSBR would be have to be financed by substantial tax increases at a later date.

The other way of analysing the recovery is to refer directly to the recession itself. This style of analysis was used most clearly by the monetarist economist Tim Congdon. For Congdon, what is important is that the recession has pushed the economy a long way from its long run trend position, and that the disequilibrium thereby created should ensure recovery. Although the devaluation is important in this account, it is not the cause of the recovery; if anything, the disequilibrium is the cause of the devaluation. The distance of the economy from its trend or long run position is measured by the difference between actual output and trend output. According to Congdon this 'output gap' is, as shown below, 'exceptionally large':

Congdon In the last two-and-a-half years, the British economy has been through a severe recession. Although the recession has been less intense than that of 1980 and 1981 (i.e. the average quarterly fall in output has been smaller this time), it has lasted significantly longer. On the assumption that underlying trend growth has remained in the normal 2-2.5 per cent area, the excess of

> trend output over actual output - the so-called "negative output gap" - must be exceptionally high. Calculations at Lombard Street Research Ltd. suggest that the negative output gap (expressed as a proportion of trend output) is currently about 6 per cent. This figure is not far from the OECD's estimate in its December Economic Outlook that the UK's negative output gap in the fourth quarter of 1992 was 6.9 per cent. The excess of trend output over actual output at present is probably the largest in the post-war period[12].

What the existence of a large 'negative output gap' means (for Congdon) is that the economy may grow at an above trend rate for several years (i.e. until actual output equals trend output) before the trade-off between growth and inflation becomes adverse. Of course, once the negative output gap has been closed, then growth must remain at or about the trend level unless productivity improves dramatically. However, given the size of the output gap, even with GDP growth of 3.5 to 4 per cent it would be 3-4 years before inflationary pressures would be a significant problem. In fact, according to Congdon, the price level may actually fall unless the government acts to ensure economic growth, so strong are the deflationary forces acting upon the economy.

Interestingly, and unlike Britton and Currie, Congdon believes that 'it is still rather early to assess the effects of the large fall in interest rates since Britain's departure from the European exchange rate mechanism on 16 September [1992].'[13] Thus, in sharp contrast to the National Institute, which feels confident enough about the effects of the devaluation to use it as the basis for their forecast, things are not so clear cut for other economists.

The other monetarist on the Panel is Patrick Minford but he has a rather different view to that of Congdon. Like Congdon, Minford believes that the economy is now a long way beneath trend and is likely to stay there unless prompt action is taken to ensure recovery. Because of this he, like Congdon, believes that inflationary pressures are weak and that above trend growth will not be inflationary. However, Minford believes that the reductions in interest rates following the devaluation are not enough to ensure recovery. Consequently:

> *Minford* The latest Liverpool forecast is for very weak growth in 1993 in the UK, relying on further falls in interest rates to the 5-6 per cent range[14].

However, Minford's reasons for believing this are slightly different from Congdon's. As shown above, Congdon uses the negative output gap to assess the inflationary pressure, or lack of it, in the economy. Minford's analysis, on the other hand, is based on the difference between the level of unemployment and its 'natural rate'. Minford believes that, because unemployment is above the 'natural rate' inflationary pressure are weak. Clearly however the ideas are linked - as economy grows, the negative output gap is closed and the rate of unemployment falls towards its natural rate - and when the output gap is closed then the level of unemployment is (by definition) at the natural rate.[15] The most important (shared) idea is thus that of movements away from an equilibrium generating powerful forces which tend to restore that equilibrium.

Unlike Britton and Currie, who based their analysis on the past responses of the UK economy, Minford's reference points come from the contemporary economic cycle. Particularly important in Minford's argument is the United States economy where debts were very high, balance sheets very weak and where interest rates had to be cut to very low levels in order to get any kind of recovery going. Taking this as his guide to the UK experience, Minford therefore argues that the Chancellor should cut rates further in order to ensure that the recovery is sustained.

> *Minford* In the absence of US-style monetary policy this weakness will continue, with the downside risk ever-present. The argument is now shifting to fiscal policy where some are arguing for a rise in taxes. However, this would be a mistake. It is the weakness of the economy mainly that has destroyed the Governments finances. The appropriate remedy is to maintain tax rates and to proceed with medium term plans to reduce public spending - focusing, as argued below, on benefits and charges for public services[16].

The remaining two economists, Gavyn Davies and Andrew Sentance, offer forecasts which draw on and combine several of these threads. For example, Davies agrees with Minford that the US experience suggests that interest rates should be cut further in order to ensure that the recovery does not stall prematurely. However, Davies's argument is based on a rather more pragmatic 'balancing of risks':

> *Davies* Policy in the next few months must be guided by an assessment of the relative risks facing the economy. In my opinion, by far the most serious risk, taking account of its consequences for the labour force, the capital stock and the government accounts, would be a prolongation of recession.

This would be much more serious than a pick-up in inflation which would probably be minor, temporary and easily controlled[17].

However, like the mainstream forecasters, Davies also believes that some acceleration in inflation, albeit unlikely, is possible. Part of the reason for this is that the negative output gap, which according to Congdon is perhaps the largest since the end of the War, might actually be much smaller than Congdon believes.

Davies The economy is now clearly working some way below capacity. A simple extrapolation of a 2 per cent growth rate since the second half of 1990 suggests that GDP in 1993 will be some 7 per below trend. However, it is possible that there has been an erosion of plant capacity in the course of the recession; Goldman Sachs' very rough estimates, based on CBI Survey data and other sources, suggests that the "output gap" may in fact be no more than 3-4 per cent[18].

If this estimate is correct then it is clear that inflationary pressures are not nearly as weak as Congdon argues. It also implies that the natural rate of unemployment is much higher than Minford (who believes the economy may be as much as 10 per cent below trend) has claimed. Thus, like mainstream forecasters Davies sees the continued high unemployment as a real possibility in the long term and as requiring significant chances in policy.

Sentance, like mainstream forecasters emphasises the beneficial effects of the exchange rate in boosting exports and hence economic growth.

Sentance The positive contribution of net trade to growth in 1993 and 1994 contrasts strongly with the situation in 1992 when net trade depressed GDP by 1.0 per cent. This turnaround reflects the impact of the more competitive exchange rate established since sterling left the ERM. The proportion of companies reporting prices as a constraint on exports in the CBI Industrial Trends Survey is now at its lowest level since the late 1970s. The pound is forecast to remain around current levels during 1993, averaging 78.2 on the effective exchange rate index (1985=100). Though sterling is expected to appreciate against other European currencies as interest rates in Europe come down and recovery is more apparent in the UK, the effective exchange rate in the final quarter of 1994 is still expected to be 10 per cent below its level in mid-1992[19].

However, unlike Britton, but like Congdon, Sentance believes that it is still too early to be sure of the implications of the devaluation:

> *Sentance* ... there has been a very significant relaxation of domestic economic policy over the last six months, the consequences of which are still unclear[20].

Finally, like Godley, Sentance believes that the balance of payments position is a cause for concern (a position emphatically rejected by Congdon, as we shall see later) and that investment will need to be boosted relative to consumption if the economic recovery is to be sustained over the longer term.

> *Sentance* One consequence of this imbalance [between consumption and investment] is that, despite the significant cuts in investment over the course of the recession, the UK ran a balance of payments deficit on current account in 1992 of 2 per cent of GDP. Even with a very modest recovery, the deficit is forecast to grow to 2.5-3.0 per cent of GDP in 1993 and 1994. Such a balance of payments deficit does not impose an absolute constraint on growth. But it does imply that if growth in the 1990s is to be sustained, it will need to be less dependent on consumer spending than in the previous decade and more reliant on investment and exports[21].

Summary

In this section I have outlined the main arguments put forward by the Panel of Forecasters in their first meeting. Britton and Currie argued that, on the basis of past experience, the devaluation should ensure recovery started. Congdon argued that it was too early to assess the effects of the devaluation, but that the negative output gap was very large and that this would ensure that above trend growth was compatible with low inflation for several years. Minford and Davies both believed that the reduction in interest rates since September were insufficient to ensure recovery and that further measures were needed. Minford however was significantly more optimistic about the medium term than Davies. Sentance and Godley were both worried about the balance of payments position and the overly high share of consumption relative to investment in GDP.

The remaining sections in this chapter set out each of the forecasters' views in more detail. The following chapters then show how these positions change and are modified, challenged and defended as the year progresses.

(Domestic) Demand

Forecasts for GDP growth in the economy can be thought of as being made up of two components. Firstly, there is the growth which takes place in the domestic economy and secondly there are the events in the world economy. This section deals with demand in the domestic economy, which is made up mainly of consumers' expenditure but also includes government spending and business investment. This latter component is particularly volatile and is thus especially difficult to forecast.

However, in 1993 there was an additional problem: how to deal with the unprecedented levels of household debt and the effect that this would have on consumers' expenditure. The following extract, from Gavyn Davies's submission, gives an idea of how uncertain economic forecasters were in February 1993. The passage, which mentions over a dozen separate indicators, also gives some idea of the things which economists look at when forming their judgements about the economy.

Davies It is possible that output stopped falling sometime in the first half of 1992, since when *manufacturing output* has been roughly flat, and *retail sales volumes* have been rising at an annual rate of about 1.5 per cent p.a. Goldman Sachs' latest estimates suggest that *non-oil GDP* was approximately unchanged during 1992Q4, while *overall GDP* probably increased by 0.2-0.5 per cent. However, it would certainly be premature to claim with any confidence that the economy is now embarked on a clear cyclical upswing. The recent strength of *M0 growth* (a much more reliable indicator of near term trends in *nominal GDP* than *M4 growth*), and the recovery in *new car sales*, are admittedly encouraging. On the other hand, the brief burst of activity in the High Street around the time of the *January sales* already seems to have petered out, and *consumer confidence* remains worryingly depressed. Elsewhere, there is plenty of evidence that the *housing market* (with the possible exception of a few straws in the wind), and the *labour market*, are still weakening. Meanwhile, *business surveys* suggest strongly rising confidence, but this has been misleading in the past, and hard evidence on the recent trend in *orders* and output suggest little or no improvement. An honest appraisal of this mixed picture is that the evidence could either be consistent with the early stages of a weak recovery, or with continued stagnation[22].

Interestingly David Currie also refers to survey evidence in his submission. However, unlike Davies, he appears to take the rise in confidence suggested by the surveys at face value and the conclusions he draws are thus rather different:

> *Currie* The latest monthly indicators, including surveys of confidence, suggest that the economy is slowly turning the corner, and that a patchy recovery is underway. Our forecast is that this recovery will slowly gather pace so that, by the end of the year, output will be growing at around its average underlying growth rate of 2.5 per cent. This gives a year-on-year growth rate this year of just over 1 per cent and around 3 per cent next year, averaging around 2.5 per cent into the medium term[23].

Note that Currie is rather more certain that recovery is underway - indeed he believes that by the end of the year GDP growth will have reached its trend rate. Of course, this is not entirely unexpected. In the previous section it was pointed out that Davies, together with Minford, was something of an outlier in that he felt that not enough had been done to ensure recovery. The complexity of his world view, illustrated in the passage quoted above, shows why he could not be sure the recovery would be sustained without further policy action. That his forecast for domestic demand was in fact relatively optimistic is due to the fact that he *assumed* the government would take action (i.e. reduce interest rates) in order to ensure that growth did occur. Currie on the other hand makes no such assumption. Without this assumption, Davies's forecast would have been much more pessimistic, as he himself makes clear.

> *Davies* Using our "main case" forecast for base rates [i.e. base rates fall to 5 per cent by mid 1993 and 4.5 by the end of the year], the GDP growth rate from 1992Q4 to 1993Q4 would be just over 2 per cent, implying calendar year growth in real GDP of about 1.5 per cent. Some further acceleration in output growth would be likely in 1994. This is currently our central forecast, though I must emphasise that it is based on the assumption of further reductions in base rates soon[24].

In other words, despite the numerical similarity of their forecasts for GDP growth (1.4 and 1.5 per cent for Currie and Davies respectively) their diagnosis of the current situation was very different indeed.

The most optimistic assessment of domestic demand was provided by Andrew Britton. In his submission, Britton emphasises that (based on past experience) one would have expected there to be an economic recovery at this time. The cuts in interest

rates since September continue a series of previous interest rate cuts stretching back over two years and should therefore add strength to the nascent recovery.

> *Britton* Normally we would not expect cuts in interest rates to act at all quickly to stimulate domestic demand, either from consumers or business. If there is indeed the beginnings of a revival in the housing market and some sectors of consumer spending this is now long overdue in view of the cuts in interest rates (from 15 per cent to 10.5 per cent) made between 1990 and 1991. The effects of the more recent reduction (from 10 per cent to 6 per cent) should be reinforcing recovery throughout this year and beyond[25].

However, this assertion, that the cut in interest rates will boost domestic demand, is contested by several of the others. Unlike Britton, whose forecasts rely heavily on the model (see Chapter 4), those forecasters which pay more attention to the peculiarities of the moment, rather than the regularities of the general case, are at pains to point out that the current debt levels mean that it is possible that consumption may remain low for the rest of the year. For example, Wynne Godley argues that it is likely that both household consumption and business investment will remain flat for the rest of the year. In both cases, unusually weak balance sheets mean that, until debt levels are reduced (or asset values rise), expenditure will be restrained. Thus:

> *Godley* House prices have continued to fall in real terms and little recovery is in prospect in view of the stock of unsold, often repossessed, houses still on the market. As houses provided so much of the collateral for household indebtedness, the fall in prices will act as an additional bromide on borrowing, while having a negative wealth effect[26] on personal consumption[27].

Similarly:

> *Godley* There is an overhang of excessive commercial building - a legacy of the property boom of the late eighties - which will act as a deterrent to new investment. In addition corporations are over indebted and banks still have weak balance sheets as a result of bad debts acquired during the eighties boom which makes borrowing for business investment in fixed and working capital unusually difficult[28].

As a result of these considerations Godley does not 'expect any significant growth in GDP this year (perhaps 0.5 per cent year-on-year) [and] little acceleration in 1994.'[29] However, it is entirely possible that it is Godley's pessimism which is unwarranted and

not the optimism of Andrew Britton. The unprecedented nature of the situation means that forecasting is particularly difficult. The essence of the problem, as Andrew Sentance makes clear, is that there are two conflicting forces present in the economy, and past experience gives no guide as to which will prove the stronger:

> *Sentance* ... trends in consumer spending have been notoriously difficult to predict in recent years with traditional forecasting methods underestimating both the strength of the consumer boom in the late 1980s and the weakness of consumer spending in the recession. Consumer spending decisions are currently the subject of two conflicting influences. High consumer indebtedness, coupled with the weakness of the housing market is acting to inhibit spending while the historically low level of interest rates now established should encourage higher spending. As this combination of circumstances has not occurred before, its outcome is particularly difficult to predict[30].

Gavyn Davies, who expresses similar concerns, argues that despite low base rates savings might remain higher, and expenditure therefore lower, than an econometric equation might suggest:

> *Davies* The continuing decline in base rates, along with low inflation, should - according to our model [which consistently underpredicted the continued rise in the savings ratio in 1991-92, see para 17] exert downward pressure on the savings ratio throughout the next two years, provided that house prices stabilise. However, while the recent rise in retail sales can be taken as a good sign, and while the full effect of the recent base rate cuts have still to come through, it is difficult to be confident that the savings ratio will in fact soon start to fall. The overhang of private sector debt remains as great as ever (though debt servicing is now more comfortable), and this may have a continuing depressing effect on consumers' expenditure, relative to predictions derived from econometric equations. It would therefore be absurd to rule out the risk that the economy might stagnate for another year[31].

Despite expressing similar concerns about the difficulty of correctly forecasting domestic demand, Sentance and Davies eventually produce very different forecasts for 1993. Davies, as noted above, makes the fairly optimistic forecast that domestic demand will increase by around 1.4 per cent, although this is based on the assumption that the government cuts interest rates still further. Sentance on the other hand forecasts

that domestic demand will hardly grow at all in 1993. The principal difference between their forecasts thus seems to be their treatment of government policy. Davies assumes that the government will act so as to bring about the recovery, Sentance on the other hand makes no such assumption and, with interest rates remaining 6 per cent throughout 1993, is significantly less optimistic about consumers' expenditure growth[32].

Congdon, like Sentance and Godley is also fairly pessimistic about domestic demand during 1993, although his reasons are rather different. Congdon's argument is that, although the economy is a long way below trend and a recovery is possible, it is being held back by government policy and may even be destroyed by it. However, if policy were to change, as he assumes it will, then the recovery will indeed get underway. In Congdon's version of events, the problems are that exports (see the next section) are likely to be weak and that the growth of the money supply is too slow to ensure any real growth in GDP.

> *Congdon* The very low rate of broad money growth now being recorded, combined with the weak outlook for the world economy, argues that the current weak revival in demand will not be sustained. In practice, policy will probably change by enough to ensure that the economy does start a genuine recovery. (The latest cut in interest rates - to 6 per cent - was announced while this submission was being written.)[33]

Finally, Minford is deeply pessimistic about domestic demand, forecasting that it will contract by just over 1 per cent during 1993. The reason is that although he believes that, as in America, interest rates will have to fall to around 3 per cent in order to ensure that recovery gets under way, he does not think this will happen. Although his forecast assumes a fall in interest rates to 4.9 per cent this is not enough, in his view, to ensure recovery. He remains unconvinced by the positive indications and his forecast is therefore for continued weakness:

> *Minford* The latest Liverpool forecast is for very weak growth in 1993 in the UK, relying on further falls in interest rates to the 5-6 per cent range. We have seen some activity in the housing markets and the "sales" have been remarkable, as have December car sales (also connected with low prices, before the January notional marking up). But this activity is consistent with general weakness: sales are being displaced to low price items and times (the house market is similarly low priced)[34].

Summary

This section has looked at forecasts for domestic demand produced by the Panel of Forecasters. The most optimistic forecasts for domestic demand were produced by Andrew Britton and David Currie. Gavyn Davies was also relatively optimistic, but this was conditional on some further policy action by the government. On the same interest rate assumptions as Britton and Currie, Davies's forecast would have been significantly lower. Numerically, Tim Congdon, Wynne Godley and Andrew Sentance produced fairly similar forecasts for domestic demand, although Congdon's, like Davies's assumed some policy action to ensure growth took place. However, the numerical similarity is deceptive. Congdon's position is essentially an optimistic one as he believes that once the recovery is established the negative output gap means that it will be sustained for several years. Godley and Sentance on the other hand see the constraints being imposed on growth as a result of the balance of payments deficit and the share of consumption in GDP. Consequently, they see the future growth being dependent on achieving higher investment levels and an improved export performance. However, it should be noted that these problems are much more severe in Godley's analysis than they are in Sentance's. Finally, Patrick Minford believes that the economy, as a result of high interest rates combined with a prolonged recession, is now in a very poor state. Recovery will take place eventually, but at the prevailing rates of interest it will be very slow, much like that experienced in America. Minford believes that if demand is to grow interest rates will need to be cut to about 3 per cent. However, he does not think this will happen and his forecast is therefore deeply pessimistic.

Net Trade / Balance of Payments

The forecasts for net trade produced by the Panel of Forecasters are all informed by very similar considerations. Firstly, there are the effects of the devaluation on the UK's competitiveness and secondly, there are the markets in which the UK must sell its exports. Taking first the effects of the devaluation, these are not disputed and, in fact are hardly mentioned at all in the report. The mainstream view, from which no-one seems to dissent is given as follows

Currie The 15 per cent devaluation provides an important impetus, boosting export volumes and holding back imports[35].

Britton As on previous occasions when sterling has fallen sharply, we expect the gain in relative cost competitiveness to result in improved profit margins for exporters as well as a substantial gain in export volume ... Devaluation also

provides an incentive for import substitution. It is encouraging therefore that import volumes (on a balance of payments basis) fell in the fourth quarter[36].

The second factor in determining the strength of UK exports and the extent to which the gain in competitiveness can be transformed into economic growth is the state of the world markets. Although the world economy is growing, which implies that the export market is expanding, the European economies, into which the majority of UK exports are sold, are expected to be very weak in 1993. This means that the demand for UK exports may not be as strong as might be expected. The situation is summed up by Andrew Sentance as follows:

Sentance ... growth in the OECD area - which dominates world trade - is subject to two conflicting pressures. Growth in the US appears to be gathering momentum while some key European economies - notably Germany - are sliding into recession with little sign yet of any significant reduction in German interest rates. The pace of growth in UK export markets depends crucially on which of these two influences dominates[37].

With two exceptions (Minford, who forecasts a positive contribution to GDP of 1.3 per cent and Congdon who forecasts a positive contribution to GDP of 0.7 per cent) all the forecasts for net trade lie in the range -0.1 to 0.3 per cent. Thus the consensus of the Panel is that the weakness of European markets will tend to more or less cancel out the gains from increasing exports to the rest of the world. In other words, net trade is generally expected to make no more than a small contribution to GDP growth during 1993. This is made clear in the following comments:

Godley Although the recent devaluation of sterling will assist British exports, the developing recession in Europe will depress the demand for UK exports - probably by more than the recovery in US growth will boost it[38].

Sentance ... Net trade is expected to make a positive contribution to growth over the next two years, adding 0.3 per cent to GDP in 1993 and 0.7 per cent in 1994. Exports of goods and services are forecast to grow by 4.4 per cent in 1993 and 5.7 per cent in 1994, compared with import growth of 3.1 per cent and 3.5 per cent respectively. In 1993, the growth of exports will be held back by sluggish European markets - especially Germany - which will reduce the growth of world trade despite signs of a strengthening recovery in the United States[39].

Britton Slow growth in the world economy this year will moderate the pace at which UK exports can expand[40].

Related to net trade is the deficit on the current account of the balance of payments. The balance of payments accounts give a record of all the UK's international transactions. There are two parts to the balance of payments accounts - there is the current account and there are the transactions in external assets and liabilities. The balance of payments forecasts given by the Panel of Forecasters refer to the current account, which includes

- exports of goods and services
- imports of goods and services
- property income from abroad and paid abroad
- current transfers from abroad and paid abroad

Thus what happens to the balance of payments reflects changes in several different sets of transactions as well as changes in the exchange rates at which the transactions take place. For example, although exports will rise in volume terms as a result of the devaluation, their value may actually fall as a result of the lower exchange rate. Similarly, although there will be less imports, they will cost more under the new exchange rate. The net effect in the short term might therefore be to actually make the deficit on the current account of the balance of payments worse as the increasing cost of imports exceeds the increased revenue from cheaper exports. In the slightly longer term however the effect of the devaluation should be to reduce the deficit.[41] This is in fact the conclusion draw by several of the Panel members as shown below:

Currie The current account deficit is expected to worsen this year, rising to about £20 billion. This is partly a consequence of the J-curve effect resulting from the fall in sterling, partly a result of renewed growth. In 1994, the beneficial effects of the devaluation should work through to check any further deterioration during 1994 due to growth, and we see a small improvement in 1995, helped also by inward direct investment to the UK. But the current account deficit will remain stuck at about 2.5-3 per cent of GDP in the longer run. There are technical reasons for thinking that the true deficit is overstated, but nonetheless it is of concern[42].

Davies The current account of the balance of payments is likely to worsen this year to around £18 billion as the J-curve effects of the devaluation work through. However, on our exchange rate forecast, this may narrow slightly in 1994[43].

Godley While exports should pick up as a result of the devaluation, the deficit in the balance of payments on the current account will probably increase further in the influence of European recession and the "J-curve effect", reaching 2.5-3 per cent of GDP in 1993 and 1994[44].

Sentance The deficit on the current account of the balance of payments is expected to widen to £16.7 billion (2,7 per cent of GDP) in 1993, as import prices offset the favourable volume movements, before narrowing slightly in 1993[45].

However, these arguments are not accepted by everyone. As noted above, the two monetarist economists, Minford and Congdon, are relatively optimistic about the prospects for net trade and this is reflected in their forecasts for the current account deficit, which show some reduction in 1993. Minford is the most optimistic as he believes that export prices will rise almost as much as import prices during 1993.

In several of the passages quoted above it was suggested that the deficit on the current account of the balance of payments may turn out to be a 'problem' in the medium term if it is not reduced. The reason for this is that if the current account is in deficit then the transactions in external assets and liabilities must show a surplus, and it is this point which is at the heart of the dispute over whether or not a deficit in the balance of payments on the current account matters. Godley is probably the economist who places the greatest emphasis on the current account although several of the Panel share his views to a lesser extent, as is shown below:

Currie ... the current account deficit will remain stuck at about 2.5-3 per cent of GDP in the longer run. There are technical reasons for thinking that the true deficit is overstated, but nonetheless it is of concern[46].

Sentance Even with a very modest recovery, the deficit is forecast to grow to 2.5-3.0 per cent of GDP in 1993 and 1994. Such a balance of payments deficit does not impose an absolute constraint on growth. But it does imply that if growth in the 1990s is to be sustained, it will need to be less dependent on consumer spending than in the previous decade and more reliant on investment and exports[47].

The reason Godley believes the deficit matters is that a persistent deficit implies that the UK must be reducing its assets or increasing its liabilities, and this is a process which cannot proceed indefinitely. However, not all the Panel agree with this analysis. In particular, Tim Congdon does not believe that a deficit on the current account of the balance of payments is necessarily a problem at all. For Congdon, so long as the government's own finances are in order then it does not matter if the recovery is accompanied by an increasing current account deficit. A detailed exposition of why Congdon believes that the issue is not a serious one was given at the October 1993 conference 'The State of the Economy' organised by the Institute for Economic Affairs[48]. It is however this 'problem' which he is dismissing in the following remarks:

> *Congdon* If the Budget were indeed roughly balanced on a cyclically-adjusted basis, the Government should not be concerned about the behaviour of the current account of the balance of payments. The recovery, when it finally emerges, may be accompanied by a widening of the current account deficit. But, if that widening were to reflect the free decisions of private-sector agents, it would not necessarily be a problem for public policy[49].

Summary

In this section I have outlined the forecasts for net trade and the deficit on the current account of the balance of payments. The view of the majority of the Panel was that net trade would add little to GDP in 1993 and that, as a result of the devaluation, the deficit on the current account of the balance of payments would increase during the year. However, in 1994 the deficit would reduce, but only to about 2.5 to 3 per cent of GDP. Several of the Panel members considered this to be a problem, although it was only a critical one for Wynne Godley. Minford and Congdon on the other hand interpreted developments rather differently. Both forecast net trade making a positive contribution to GDP, with Minford forecasting that net trade would add 1.3 per cent to GDP. Congdon was less ebullient but still forecast a contribution of 0.7 per cent, more that two times anyone else's forecast.

These forecasts for net trade are also reflected in the Panel's forecasts for the current account deficit. For Congdon, who forecast a sharp fall in the rate of increase of imports relative to exports, the balance of payments deficit does improve during 1993, albeit slightly. Minford, who does not forecast exports and imports separately, is also very optimistic about the deficit and forecasts that it will fall sharply. The rest of the Panel see imports increasing less rapidly than exports, but the difference is not enough

to offset the change in the exchange rate. Consequently their forecast is for the deficit to increase from £11.8 bn in 1992 to anywhere between £16.7 and £20.2 bn in 1993 (i.e. an increase of 40 to 70 per cent).

Inflation

Although, as shown in the previous sections, there are differences between the Panel on almost all issues, it is the forecasts for inflation which show the greatest differences. The monetarist members of the Panel, Minford and Congdon, can be termed 'inflation optimists' whilst the rest of the Panel are 'inflation pessimists'. When discussing the general theoretical views which informed the Panel's forecasts, it was noted that Congdon's estimate of the negative output gap was comparatively large. This, combined with the high level of unemployment, leads Congdon to the following conclusions:

> *Congdon* The large excess of trend output over actual output and the high level of unemployment therefore argue that the downward pressures on inflation are unusually strong. It is a matter for econometric testing whether the change in the inflation rate is a function of the rate of change in output (relative to trend) or the level of output (relative to trend). An inflation equation used at Lombard Street Research suggests that the level of output relative to trend is likely to be far more significant than the rate of change of output, for any plausible values of these variables. (This result is of course consistent with the model of wage determination developed in Friedman's 1967 presidential address to the American Economic Association and the subsequent literature). If the equation and the large negative output gap are considered together, the implication is that above-trend growth can be reconciled for several years with low inflation[50].

Congdon's argument is thus based on a theoretical analysis backed by empirical econometrics. The theory is also important when it comes to weighing up other evidence. Note that in the following discussion, the positive indications from other sources are subordinate to the effects due to the negative output gap:

> *Congdon* However, the January CBI survey (with a positive balance of companies on the "output expectations" question) and the December Building Societies Association press release (with a sharp increase in seasonally-adjusted mortgage commitments) point to a resumption of growth in the first half of 1993, although at a beneath-trend rate. Although the pound may fall further

and the devaluation will have an impact effect on the price level, the negative output gap will continue to increase again and underlying inflationary pressures will continue to weaken[51].

Thus, despite growth in the economy, the negative output gap will continue to increase throughout 1993, albeit at a slower rate, and inflation is therefore expected to remain low and to fall further (to just 1 per cent) during 1994. Minford's analysis is basically similar, relying on the pressure of high unemployment and tight public spending, although he is not as optimistic as Congdon. Nevertheless, although he does not forecast inflation to be as low as Congdon projects, Minford is still classed as optimistic. The reasons for this are as follows: although his forecast is for underlying inflation to reach 3.8 per cent in 1993 is only just below the average of 3.9 per cent, his submission does not identify inflation as being a problem. Indeed, Minford's major anxiety is not that inflation will take off but that the economy will fall back into recession if growth does not resume soon, so weak are the inflationary pressures. It is in this sense that Minford is optimistic about inflation.

The rest of the Panel are distinctly less optimistic, although some are more worried than others. However, the reason why the group as a whole is pessimistic is that they do not see any great improvement in the economic situation *before* inflation becomes a problem. In other words, for this group, the growth-inflation trade-off is much more adverse than for Minford and Congdon, leading to forecasts of inflation remaining above 3 per cent, combined with continued high unemployment.

The most pessimistic forecast for inflation in 1993 was produced by Wynne Godley, who forecast that inflation would rise to 4.8 per cent by the end of the year. However, it should be noted that this mainly reflects the devaluation and the rise is temporary.

Godley As the effect of the devaluation comes through, price inflation (excluding mortgage interest payments) will probably rise to nearly 5 per cent but this increase is likely to be transitory and should not, therefore, dilute the effect of devaluation on British competitiveness. The size of wage settlements is falling fast and with high and rising unemployment there seems little danger of an acceleration in domestic costs in the immediate future[52].

In 1994, Godley's forecast for inflation falls to around 4 per cent, which is closer to the average of 3.6 per cent. However, it should be noted that this is in the context of below-

trend growth and rising unemployment. Taking the forecast as a whole, Godley is therefore deeply pessimistic.

If we look at the trend of the inflation forecasts rather than their numerical value, then the most pessimistic inflation forecasts are those of Britton and Currie. Both Britton and Currie offer a very similar analysis, based on a predicted rise in wage demands following the devaluation, and both see temporary breaches of the official inflation target of 1-4 per cent as likely. Currie's forecasts are as follows:

> *Currie* ... we see inflation remaining low over the next year, but then picking up as a consequence of devaluation. Headline RPI inflation averages about 2.5 per cent this year, partly as a result of cuts in mortgage rates, but then picks up. The Chancellor's target indicator, RPI excluding MIPs, remains at the top end of the target range during 1993, but then rises above it in 1994[53].

The predicted rise in wages, which leads to the relatively high inflation forecast is described as follows, with heavy emphasis on the 'typical' as opposed to the 'particular':

> *Currie* ... If higher inflation feeds into higher wages, there is the danger that the competitive advantage of a lower pound is eroded in a wage/price spiral: this occurred in the three years after the 1967 devaluation, and is the characteristic response of the British economy. Over the next year, with unemployment high and rising, we see no such danger. But in 1994 and beyond, with unemployment stabilising and growth at its trend rate, we see earnings responding to higher inflation, and higher interest rates are needed to stem rising inflation[54].

Thus, Currie is clearly pessimistic about the ability of the economy to expand without increases in inflation which result in deflationary policies, and this is the reason for the 'inflation pessimist' label. Of the other mainstream forecasters, Andrew Britton's forecast is slightly higher than Currie's for 1993 but lower in 1994. However, like Currie, Britton forecasts headline inflation of above 5 per cent in 1994. Their forecasts of underlying inflation are also the highest. Again, it is interesting to note the prominence given to the 1967 devaluation in the National Institute's forecast, although some allowance is made for the exceptionally high unemployment in 1993:

> *Britton* Devaluation has already raised import prices by 9 per cent in the fourth quarter. That in turn must add to wage pressure, although not necessarily

straight away. After the 1967 devaluation wages were held back for a year or so by a successful episode of incomes policy, but there was a sharp rebound in 1970. By the end of 1971 the whole of the devaluation gain in competitiveness had been reversed. The conditions of 1993 are of course very different, not least because unemployment is so much higher. Even so we would expect to see wage settlements beginning to rise again in the course of this year, provided that the output recovery is significant and sustained. This is the reasoning behind our forecast that the target band for inflation may well be exceeded[55].

The rest of the Panel forecast inflation somewhat lower than Britton and Currie, but do not take the effect of the negative output gap to be as pronounced as Congdon. Sentance ultimately gives the edge to high unemployment, assessing the two opposing forces as follows:

Sentance The pace of growth anticipated over the next two years is insufficient to reduce unemployment, though the total is expected to rise more slowly, reaching 3.2 million by the end of 1994. With the labour market so slack, low earnings growth and rising productivity should offset the impact of the depreciation on import prices, keeping down inflation. The latest deceleration in manufacturing pay settlements - to 3.1 per cent in the three months to November - is entirely consistent with this view. Underlying inflation (measured by the RPI excluding mortgage interest) is expected to fall gradually to 3.1 per cent by the end of 1993. Though there may be some upward pressure of prices in 1994 as the economy recovers, underlying inflation is expected to remain within the Chancellor's target range of 1-4 per cent averaging 3.5 per cent in the final quarter[56].

Davies on the other hand seems to give slightly more weight to the effect of the devaluation:

Davies On inflation, leading indicator and structural models suggest that the underlying rate may remain close to the, or perhaps slightly above, the top end of the 1-4 per cent target band for much of 1993, before declining again next year. The adverse effects of the sterling devaluation on inflation are quite large, but are offset by the lagged effects of the recession, especially on service prices. We expect unit labour costs in the whole economy to be unchanged both this year and next, in which case the underlying inflation rate may fall back modestly in 1994[57].

Summary

In this section I have drawn a distinction between 'inflation optimists' and 'inflation pessimists'. The inflation optimists were those forecasters who predicted that inflation would fall or, at the very least, that inflation would not rise as economic activity increased. The pessimists either predicted that inflation would rise significantly over the year and into 1994 or that if inflation did not get worse then nothing else would get much better. The labels thus referred not just to the numerical values forecast for inflation, but also to the economic context within which those forecasts for inflation were embedded.

According to this division, Congdon was the most optimistic for inflation, although Minford also foresaw 'reasonable' medium term prospects coupled with falling unemployment and above-trend growth. Of the rest of the Panel, Britton and Currie forecast that inflation was on a rising trend and were, numerically, the most pessimistic. However, Godley also forecast inflation of 4 per cent in 1994 and, when viewed in the light of his forecasts for an extremely weak expansion of demand, it is clear that Godley is also very pessimistic. In Godley's case however, the pessimism is unmodified by reference to anything in particular - Godley just is pessimistic.

Finally, Sentance and Davies, see both the optimists' and pessimists' points as worthy but find themselves unable to side wholeheartedly with either positions. Their forecasts and their analysis lie therefore somewhere in between the more extreme views.

Unemployment / Wages

The forecasts for unemployment are basically very similar at the start of the year. Britton, Currie, Davies and Sentance all predict that unemployment will increase slightly from 2.9 million at the end to 1992 to about 3.2 million by the end of 1993. In 1994, they forecast that unemployment will fall back slightly but will remain at or above 3 million. The reason why unemployment does not fall, despite increasing economic activity is that any expansion of demand sufficient to reduce unemployment will increase inflation to such an extent that deflationary policies will be required. However, deflating the economy to reduce inflation will increase unemployment. It is therefore, as David Currie observes, an 'intractable' policy problem:

> *Currie* Unemployment is likely to rise to 3.25 million this year and to decline only slowly thereafter, dipping only marginally below 3 million in 1996. This is an intractable policy problem. The danger is that the policy response will be

to go for growth to bring unemployment down, raising inflation and pushing the UK into another inflationary boom/bust cycle[58].

The remainder of this consensus group offer very similar forecasts to Currie, and for very similar reasons. Thus:

Britton The worst news since our [i.e. the NI's] last Review was the sharp rise in unemployment in the fourth quarter. This follows from the sharp fall in employment now recorded for the third quarter of last year, a time when business confidence was at its low point. The same surveys which now indicate hope of a recovery in output predict further job losses this year. We have therefore raised our forecast of unemployment, with the peak now predicted at around 3.2 million [in] the first quarter of 1994[59].

Davies Such a profile for output [1992Q4 to 1993Q4 = 2 per cent, annual rate = 1.5 per cent] would lead to unemployment rising to about 3.2 million by the end of 1993, possibly flattening next year[60].

Sentance The pace of growth anticipated over the next two years is insufficient to reduce unemployment, though the total is expected to rise more slowly, reaching 3.2 million by the end of 1994[61].

Congdon's forecasts for unemployment are slightly higher than those of this mainstream group. However, the reasons for this are difficult to judge because he literally does not mention unemployment in his Submissions (apart from the remarks already cited in which the existence of high unemployment is used to buttress the conclusion that inflationary pressures are very weak). This is slightly unusual in that unemployment is usually one of the 'Big four' variables economic forecasters are expected to forecast accurately. However, its relative neglect as a policy topic in Congdon's Submission reflects not so much its minor role in his own analysis but its unimportance, in his view, as a matter for public policy. Put simply, Congdon does not believe that ensuring full employment is part of the responsibility of governments.[62] With this in mind, it is perhaps understandable that unemployment is not mentioned in his policy submission.

If Britton, Congdon, Currie, Davies and Sentance represent the central tendency of the group, then Wynne Godley and Patrick Minford are the outliers. Godley's is the highest forecast for unemployment and is entirely consistent with his bleak forecasts for activity:

Godley If output rises 1 per cent or less during 1993 and 1994, unemployment (in the absence of any special new employment measures or changes of definition) is likely to go on rising fast, perhaps approaching 3.5 million by the end of this year with further increases thereafter. Even if output were to rise rather more than 1.5 per cent per annum, unemployment could go on rising for several years. (It may be recalled that unemployment rose by a million between 1981 and 1986 although the average growth of GDP during that period was just over 3 per cent per annum.)[63]

Minford on the other hand represents the more optimistic view of unemployment. In the short term, he thinks that unemployment will continue to rise as the economy is weak and growth is likely to be sluggish in the absence of interest rate cuts:

Minford Unemployment is rising rapidly still, and companies are cutting costs sharply, having finally given up on any sort of rapid recovery[64].

However, in the longer term, once the recovery has started, Minford is much more optimistic about unemployment. This optimism is based on Minford's view of the UK labour market and the effect of the recession. Minford believes that the economy may be as much as 10 per cent below trend in terms of output.[65] Similarly he believes that the 'natural rate' of unemployment, i.e. the level below which inflationary pressures will begin to assert themselves, is much lower than the consensus view. Thus he writes:

Minford It is usual in recessions - and the worse they are, the more it is done - to mark down estimates of "potential output" and mark up those of the "natural rate" of unemployment. These estimates are however generally little better than extrapolations of recent trends[66].

However, even those economists who do analyse the labour market differ about how competitive it really is, and thus about the natural rate of unemployment. As Minford makes clear, despite their different starting points, both types of analysis reach the conclusion that the natural rate of unemployment is determined by the same sorts of things:

Minford There are two available types of analysis of this [i.e. the labour] market. One (as in my own work) assumes that at least in a significant part of the labour market there is a competitive supply and demand for labour: there are other parts that are either monopolised by unions or monopolised by firms, but anyone failing to find work in those must look for it in the competitive part. The other type of analysis (pursued in many papers by Professors

Steve Nickell of Oxford and Richard Layard of LSE) assumes that throughout the labour market firms bargain with unions or other worker groups for wages and set prices in response[67].

Minford Both types produce similar conclusions: that unemployment benefit (its rate and availability), taxes and union power are the principal determinants of unemployment[68].

What sets Minford apart from the rest of the Panel, who more or less follow the Layard-Nickell line on unemployment, is the effectiveness of government policies in changing the labour market in the UK. The rest of the Panel (with the possible exception of Congdon) believe that previous labour market policies have not had much effect and that, for a variety of reasons, the natural rate of unemployment is around 2 to 3 million. This, of course, is perfectly consistent with their view that any upturn in activity will quickly lead to inflation. Minford, on the other hand, draws very different conclusions:

Minford Applying this sort of thinking to the UK one is struck by how massively the labour market environment in this sense has changed since 1979, when both approaches suggested that the UK natural rate of unemployment would be high - up to 3 million or 11 per cent. Cuts in benefit rates relative to productivity-linked wages, tougher eligibility for benefit including the renewed Beveridge "worktest" as in the "Restart" scheme, union laws, cuts in marginal tax rates, and so on. The Liverpool estimate of the natural rate that results from applying these ideas is down to around 1 million - approximately 3.5 per cent of today's labour force[69].

It is on the basis of this estimate that Minford's optimistic projections for demand, inflation and unemployment rest. Put simply the economy is well below trend and, as the output gap is to be closed, unemployment will fall. Inflation will not be a major problem however because of the strong competitive forces in world markets and the much increased efficiency (insecurity?) of the domestic labour market.

Summary

This section has focused on unemployment. The majority of the Panel forecast a slight rise in unemployment in 1993 and little change in 1994. In addition, for those that see unemployment as a policy problem, it has to be said that they see it as a very difficult one to resolve. The difficulty stems from belief that the natural rate of unemployment is very high (i.e. close to the prevailing level of unemployment) and that any above trend growth which reduces this unemployment will quickly lead to inflation and deflationary

policies. Godley, who forecasts less growth than this group, is the most pessimistic, and forecasts unemployment rising to above 3.5 million

Minford, on the other hand, uses a very different form of analysis to the 'Layard-Nickell' model adopted by the rest of the Panel. His model, which is based on the assumption of strong competition in the labour market, leads him to the conclusion that the natural rate of unemployment is as low as 1 million. Given this, it is clear that the economy is a long, long way below trend and that unemployment can fall substantially before any inflationary problems are encountered. Indeed, it follows from this estimate of unemployment that the economy may be as much as 10 per cent below trend, and this is why Minford argues that interest rates need to be cut.

PSBR

The Public Sector Borrowing Requirement (PSBR) is the amount by which the gross government debt increases during the financial year. The PSBR is related to the deficit or surplus (i.e. income less expenditure) of the government but not in a mechanical way. The relationship is made complicated because, in addition to buying goods and services, the government also makes loans and sells assets (although the latter are also presented separately). Thus during the year some assets might be sold or privatised, a number of new loans made and some of the existing ones repaid. The size of the PSBR matters because a large PSBR implies an increasing government debt. As this debt increases so the interest payments which have to be made on it also increase and, as the debt becomes large, they can increase explosively. In addition, a ratio of debt to GDP of less than 60% was one of the criteria for monetary convergence set by the European Union. For Gavyn Davies, despite the events of the previous summer, these criteria remain important (or at least useful) as guidelines for assessing policy:

> *Davies* On the growth and inflation assumptions [growth 3 per cent, inflation 3-4 per cent] just mentioned, the PSBR seems likely to rise to about £55-60 billion in 1995-6, falling back to perhaps £50 billion by 1997-98. This would result in the ratio of government debt to GDP rising to over 60 percent by 1995-96[70].

Finally, the increase in public spending which had brought about the large PSBR was an expansionary policy. Whilst such a policy was wholly appropriate during the recession, it was now, with recovery underway, a cause for concern:

Currie There are distinct dangers with this large overhang of government borrowing. First, there is the worry that high government borrowing coupled with low interest rates will prove unduly expansionary and inflationary in the medium to longer run[71]

Insofar as the debate about economic policy is concerned, what matters about the PSBR, which in February 1993 was rather large, is the extent to which it will be reduced by the increase in economic activity. If, for example, the large PSBR, which all agree was created at least in part by the recession, is wiped out as the economy expands then clearly the PSBR is not a problem for policy - it will go away all by itself. If this is indeed the case then the PSBR is said to be 'cyclical' as it is purely a product of the economic cycle. On the other hand, it is possible that despite renewed economic growth, the PSBR will remain at its current level. If this is the case, then the PSBR is a problem for policy. If this is the case then the PSBR is said to be 'structural'. As noted in the previous paragraph, a large deficit which does not go away can lead to other problems. The concerns of the majority of the Panel members are most clearly expressed by David Currie:

Currie Although part of the PSBR is indeed cyclical, perhaps as much as 2-3 per cent [of GDP], the structural deficit is as much as 4-5 per cent [of GDP]. On unchanged budgetary policies, the PSBR remains stuck at around £50 billion despite revenues rising with recovery: this is because at this level of borrowing, interest payments rise rapidly[72].

Of course, the distinction between the structural and cyclical components of the PSBR has to be judged in the context of the overall economic recovery. For example, for those that forecast a relatively slow recovery (e.g. Sentance and Godley) then the tendency for growth to reduce the deficit will be muted and the deficit will appear mostly structural. If growth were to be faster the reduction would be greater and the deficit appear more cyclical. On the other hand, even if recovery is not strong then the PSBR could still come down if taxes were increased and/or public expenditure restrained or even reduced. If these policies are applied with sufficient determination (as for example Minford assumes) then the PSBR can be virtually eliminated with only a fairly modest expansion in output. The PSBR can thus become either cyclical or structural as a result of policy; it is not entirely predetermined. It is thus a matter for judgement on the part of the forecaster as to how the economy will grow, how the government will act, and how these interact with each other.

Sentance ... the high level of the Public Sector Borrowing Requirement, which is expected to reach £45.2 billion in 1993-94 and £50.4 billion in 1994-95, implies a continued restraint on public spending. Government consumption is forecast to rise by just 0.5 per cent in 1993 and by the same amount in 1994. *No significant tax increases are assumed in the forecast but they are clearly a possibility*[73].

In practice nobody held the view that the PSBR was purely structural, although Patrick Minford did argue that it was (could be made to be?) almost purely cyclical. The reasoning, as can be seen from the passage quoted below, is based on his estimate of the natural rate of unemployment and a 'negative output gap' of 10 per cent. However, it is also worth noting that Minford also assumes a fall (of 3 per cent) in government consumption in 1993-94. When all these factors are combined, we get a 'back of the envelope' calculation of the PSBR as follows:

Minford These figures [i.e. natural rate of unemployment ≈ 1 million, output can rise by approx. 10 per cent from 1993 level] can also be translated into some estimates of PSBR impact: £30-40 billion of the projected 1993 PSBR is due to the recession. The marginal tax rate paid on the wages of the average worker today (including VAT, all National Insurance and direct tax) is about 47 per cent. On top of this, for every £100 million lost in wage bill, there is probably another £10 million lost in corporation tax (since the Lawson reforms, a highly geared tax), as trading profits fall £50 million: national income basically splits one third trading profits, two thirds wage bill. Finally, there are unemployment benefits, costing the Treasury another £20 million per £100 million loss in wage bill. Hence, every £150 million reduction in national income costs the Exchequer about £77 million, or just over 50 per cent (the Treasury in a recent Bulletin has put it even higher at 70 per cent - this is entirely possible, given the difficulties of assessing precisely how recent tax changes have affected PSBR "gearing" to the economy). National income in 1993 looks like being some 10 per cent lower than its potential, implying an Exchequer loss of £30-40 billion - even more on the Treasury's estimates of the PSBR's gearing to national income[74].

Consequently, the PSBR is mostly cyclical and no special measures (apart from the assumed restraint in government consumption and investment) are necessary to eliminate it. However, as noted in previous sections, many of the Panel believed that (a)

the output gap was smaller than Minford's estimates and (b) that the natural rate of unemployment was higher. Thus, most of the Panel believed that the PSBR was only partly cyclical and that although the increase in economic activity would reduce it, it would not eliminate it. There was thus a significant structural component in the PSBR which the majority of the Panel thought would remain despite the increase in activity.

> *Currie* In framing his March and December Budgets, the Chancellor has to steer between the problems of a tentative recovery and rising unemployment and the problems posed by the PSBR heading towards £50 billion, or 7 per cent of GDP, in 1993-94 and 1994-95. We reject the view that most of this public sector deficit is a cyclical phenomenon that will disappear with recovery[75].

Because of this most of the Panel recommended that taxes should be increased. With the sole exception of Congdon, all those advocating tax increases wanted them to be deferred until the recovery was clearly underway. Others felt that the recovery was still too uncertain to risk increasing taxes and thereby stalling economic growth. The problem is that both households and business have very weak balance sheets due to large debts. They are therefore extremely reluctant to spend and are saving a significant proportion of their income. As the economy expands this unusually high propensity to save might well come down. However, if taxes are increased too soon then the capacity of the private sector to spend will be reduced still further and this is a most undesirable outcome at such an early stage of the recovery. The strongest and most consistent statement of this argument was given by Wynne Godley:

> *Godley* Recent forecasts of a ballooning PSBR during the nineties are all based on the assumption that the exceptionally large private sector surplus in 1992 will be repeated in 1993 and increased further in the years to come. This has to be the case because the sectoral balances must, by identity, sum to zero. A PSBR equal to 9 per cent of GDP logically implies, since no-one expects the balance of payments deficit to be more than 5 per cent, that there will be a private sector surplus equal to 5 per cent or more. But private financial surpluses of this size have only been approached in the past in periods of very high inflation such as the seventies and the only reason for expecting them now (when inflation is low) is that, in the aftermath of the (collapsed) credit and property boom, corporate and personal saving will for several years, but not indefinitely, remain exceptionally high relative to

investment. It would be perverse in the extreme to respond to this exceptionally low private spending relative to income by fiscal restriction[76].

However, it should be noted that one reason why Godley can hold this position so forcefully is that his own theoretical approach leads him to very different conclusions about the urgency with which the PSBR must be reduced. Thus, unlike the rest of the Panel who, as noted above, regard the large PSBR as a problem which requires attention, Godley argues that:

Godley It is a major contention of this memorandum that the mere existence of a large PSBR generates no requirement whatever to raise taxes (or cut public expenditure) additional to what is needed as a result of some supply constraint such as the balance of payments, accelerating inflation or shortage of capacity or labour[77].

The reasoning behind this argument is outlined below. Probably the most important point to notice is that the argument only applies if the economy is entirely demand constrained. Clearly Godley believes that it is, and will remain so for some time.

Godley If an economy is entirely demand constrained, it will tend, following an increase in public expenditure, towards a stable equilibrium, with no explosion of interest payments, whatever the initial state of the government's finances might be. The intuition is that so long as there is no supply constraint of any kind an addition to public expenditure will eventually add enough to national output to generate enough tax revenue - at given tax rates - to cover not only the initial addition to public expenditure but also the interest payments which have been generated by the additional public sector debt ... there is no reason (arising from public sector indebtedness) why the government should not run a large deficit at one time and raise taxes to reduce it at another, later, time[78].

There is some doubt as to how far the other Panel members would agree with this diagnosis. However, as Godley himself pointed out no-one actually disputed the point at the time.[79]

Summary

The forecasts for the PSBR are some of the most complicated to understand because they are the result of several different judgements. Firstly, an assessment is necessary as to how far from equilibrium (a concept with which Godley would probably disagree) i.e.

from potential output and natural rate of unemployment the economy is. As we have seen, when made explicit these estimates range from a 3 to 10 per cent gap on output, and from 1 to 2.5 million for the natural rate of unemployment. Linked to these is the expected growth in the economy. As the economy expands and the output gap is closed so unemployment falls, tax revenue rises and state expenditure can fall. In addition, tax rates and spending plans may also be changed, but this depends on what is politically feasible. Forecasting the PSBR requires all these factors to be taken into account and weighed up before a final judgement is made.

In general terms, the consensus view of the Panel was that the deficit was a problem. More specifically, the deficit was a problem that would not go away (i.e. it was not cyclical) and that additional tax revenue would be needed to reduce it. However, it was also believed that this increase in taxes should be delayed until the recovery was clearly under way. In addition to this consensus view there were two significant outlying positions. Firstly, Minford argued that the deficit was not a problem because the economy was so far beneath trend that it would be eliminated automatically as the output gap closed (assuming a tight squeeze on public expenditure also took place at the same time). Finally, Godley also believed that the PSBR was not a problem in the sense that action was required to reduce it.

Budget / Policy (Fiscal)

Despite the variety of forecasts and analyses outlined above, the Panel reached a reasonably clear consensus on what the appropriate policy responses were. Although I have split the discussion into fiscal and monetary policy it is important to remember (as was made clear in Chapter 2) that they are actually determined with respect to each other. The tightening of fiscal policy implies a loosening of monetary policy if the overall effect is not to be deflationary, for example. Amongst the Panel there was a consensus that fiscal policy would need to be tightened over the next few years. This tightening might take the form of increased taxation, reduced spending or a mixture of both. However, there were differences as to when this tightening of policy should take place and why it was actually needed.

Taking first the timing, Congdon believed that, because the longer any tax increase is delayed the larger it has to be, it was better to raise taxes immediately. However, the rest of the Panel disagreed, arguing that there was a risk that deflationary policy measures might bring the recovery to a halt. Clearly such an outcome would be

very bad for the economy and so the majority argued that any tightening should not take place until the recovery was more securely established:

Currie All th[e] arguments point to the need to get public borrowing down. But it is argued that higher taxes now will stall recovery. I am a little doubtful of this in view of the major shift in policy since September, but the Chancellor can avoid this risk while still addressing in this Budget the problem of excessive government borrowing. Since a structural deficit of some 2 per cent of GDP is acceptable, the Chancellor should aim at a fiscal adjustment of some 2-3 per cent of GDP over the next few years[80].

Congdon Th[e] easing of monetary policy should be accompanied by a tightening of fiscal policy, because the present level of the public sector borrowing requirement carries long run dangers of excessive debt accumulation and a rising burden of debt interest payments. If public expenditure cannot be reduced, an increase in taxation equivalent to 2 per cent of GDP should be announced in the March Budget to take effect over the next year or two[81].

Davies Th[e] monetary easing should set the stage for an explicit and significant tightening in the fiscal stance. This should not actually take effect this year (indeed, there should be an urgent package of measures to help the long term unemployed), but measures should be spelled out in the forthcoming Budget to tighten fiscal policy by at least 2 per cent of GDP, and preferably more, over the next 3 years. In the absence of large-scale expenditure cuts, the overall aim should be to introduce tax increases which are anyway desirable from the perspective of tax reform (e.g. a wider VAT base, petrol and tobacco taxes, environmental taxes, the phasing out of mortgage relief etc.) In the optimistic case that the PSBR drops faster than expected, these changes could be offset by income tax cuts, in which case a desirable tax reform would have taken place. However, if a coherent plan of this sort is not spelled out this year, this will only invite wild media speculation about future tax rises. The outcome for activity could be worse than saying nothing about tax increases in the Budget[82].

Sentance While there is still considerable uncertainty surrounding the way in which consumers will respond and the prospects for recovery are so uncertain, it would be wrong to raise taxes in the forthcoming Budget. This would give the wrong signal at a time when confidence is still fragile ... Only when it is

clear that an increase in spending is firmly established should the Government consider tax changes to reduce public borrowing[83].

In addition, Wynne Godley who, as we saw in the previous section, argued strongly that the existence of a large PSBR was not, in itself, cause for raising taxes also argued that tax increases in the next few years would be desirable. Godley's recommendations are based on his belief that 'domestic demand must be substantially reduced as a share of GDP'. The following passages outline why this is so:

Godley Investment and some kinds of public expenditure (for instance on education and school maintenance) should be increased relative to consumption as a share of domestic demand. Such a shift is needed to make the economy more competitive; it would also reduce the import content of demand as a whole, thereby generating a higher level of employment corresponding to a zero (or any given) balance of payments[84].

Godley But policies to improve net export demand, while necessary are not sufficient to bring about the needed structural changes. Such policies can only be successful if domestic demand is substantially reduced as a share of GDP. For this reason - nothing to do with the PSBR as such - I am in full agreement with those who think that a substantial "fiscal adjustment" of a restrictive kind will be necessary at some stage in the medium term although the difference of analysis may lead to differences of view about scale and timing. If my forecast for flat consumption this year is correct it would be unnecessary, and indeed quite wrong, to raise taxes immediately since this would have a damaging effect both on employment and on business confidence. It is not yet clear whether there will even be any need to raise taxes a year from now[85].

This line of reasoning was also used by Andrew Sentance:

Sentance ... consumer spending will need to grow much more slowly in the 1990s - at or below the 2 per cent per annum trend rate of growth the economy sustained in the 1980s. There are two ways in which this might be achieved. The first is through the natural caution of consumers as they seek to run down the high levels of debt built up during the 1980s and during the recession. This is likely to be more pronounced in the early stages of the recovery, particularly if unemployment continues to rise. But if consumers are less cautious - which is likely if the recovery becomes strongly established - tax increases may be needed to contain consumer spending to

avoid pushing up interest rates significantly and damaging investment prospects[86].

Finally, there were also a minority of economists on the Panel who called for the government to adopt policies designed to reinforce the recovery. As shown above, Gavyn Davies argued that 'there should be an urgent package of measures to help the long term unemployed'. In addition, Wynne Godley and Andrew Britton also made similar recommendations, although only Wynne Godley calls for an increase in expenditure:

Britton The balance of risks points to a neutral Budget in March. I would especially advise against increases in indirect taxes at that time, because they would have a relatively quick effect on consumer spending and because they would increase prices. Within that neutral Budget more could be done (and should have been done some time ago) to ease the severity of the employment situation. Central and local government, and public corporations, should be delaying any measures which involve laying off workers until the recovery is well established, whilst bringing forward measures which involve taking on extra staff[87].

Godley For the same reason that there is a case against raising taxes at once there is, in my view, a strong case for adding immediately to certain kinds of public expenditure, particularly on construction, in order to reinforce the recovery. If ever we achieve a "rebalanced" economy, with the various sectors in desirable and sustainable relationships to one another, the building industry will be enjoying a much higher level of activity than at present. There is, accordingly, no valid reason for delaying the recovery in demand for building and construction as this vital sector remains dangerously depressed[88].

Summary

This section has outlined some the major fiscal policy recommendations of the Panel of Forecasters. Despite the range of analyses and forecasts there was a reasonably clear consensus that some form of fiscal tightening should take place over the next two to three years. On balance it was felt that the risk to the recovery was too great to justify tightening policy right away. Of the two economists who did not explicitly call for a further tightening of policy it is worth noting that Minford actually assumed such a tightening would take place. Finally, Andrew Britton, who called for a neutral Budget

in his submission, remained agnostic about the need for further fiscal tightening, arguing that if the tax increases were to take place in the following financial year, then the current Budget was not the appropriate time to announce them.[89]

Budget / Policy (Monetary)

As was pointed out in the previous section, the counterpart of fiscal policy is monetary policy. The obvious implication of the restrictive fiscal policies outlined above is that monetary policy should be relaxed if deflationary effects are to be avoided. However, the evidence suggests that changes in monetary policy may take 1 or 2 years before their effects are fully felt. Given that interest rates have fallen from 10 to 6 per cent in the previous six months and that the exchange rate has depreciated by 10-15 per cent, there is already a considerable monetary relaxation 'in the pipeline', the effects of which will be felt during 1993-94. Consequently, it is not at all clear whether interest rates should be raised, lowered or left where they were.

The monetarist members of the Panel, Minford and Congdon, are both reasonably clear that monetary policy needs to be relaxed, although the measure of monetary policy they use differs. For Minford, the appropriate measure is the real interest rate, for Congdon it is the growth of the money supply. Nevertheless, both are clear that monetary policy needs to be more expansionary:

> *Congdon* The implied recommendation is that the authorities should raise monetary growth in the next year to 18 months, in order to halt the recession, but they should then gradually reduce monetary growth thereafter. The right figures are a matter of judgement, but broad money growth approaching 10 per cent over the next year would not carry any early inflationary risks[90].
>
> *Minford* At present [monetary] growth is too low: M0 has recovered very recently but M4 remains exceedingly weak. In the US narrow money supply measures have been buoyant for many months but M2, the broader aggregate, has failed to respond and has been an accurate guide to the weakness of the economy in this period of high indebtedness and property market collapse. Recent UK experience is as unprecedented as in the US - the closest and most disturbing parallels are with the 1930s - and we should observe US behaviour closely for guides to our own responses. It suggests that money supply growth here is too weak to sustain recovery. US rates, it must be stressed, had to fall to 3 per cent to obtain signs of growth in M2 and get a weak recovery going ... I welcome the drop in interest rates to 6

per cent. However it is insufficient given the weakness of the money supply indicators. It is desirable to move decisively to stimulate these by dropping rates to around 3 per cent as in the US and Japan[91].

Other members of the Panel also felt that some relaxation in monetary policy might be appropriate. For example:

Davies ... I would err in the direction of easing monetary policy too much rather than too little. At this stage I would favour making two more one point cuts in base rates in the next few months - though this should occur in the context of medium term budget consolidation[92].

Sentance If signs of recovery remain elusive during 1993, with inflationary pressures subdued, it should be safe to encourage growth by cutting interest rates further. Lower interest rates would ease business cashflows and encourage capital spending directly as well as assisting the process of reducing personal indebtedness. However, in the light of the very substantial relaxation in monetary policy that has already taken place, the case for lower interest rates should be judged against the evidence from the real economy - including survey data - as it emerges over the course of the year[93].

Finally, Wynne Godley also argued for a further relaxation of monetary policy, in terms of a lower exchange rate. However, given the very weak state of the economy he foresaw no need to reduce interest rates to achieve this.

Godley Present circumstances are unusually favourable for a devaluation to be successful as there is a lot of spare capacity while unemployment is so high, and rising so fast, that a response in the form of much higher money wages is rather unlikely. At the time of writing, there has been a devaluation of about 16 per cent compared with mid-1992. While a devaluation of this size, particularly under the especially favourable circumstances of 1993, will greatly help net export demand, I doubt whether it will be enough to do the trick. There is room for disagreement about the size of the change required and I do not want to be too emphatic about this. My guess is that a further devaluation of about 10 per cent might be about right and I recommend that policy should now be directed towards achieving this result. I emphasise, however, that it is not at all clear to me that any further reduction in nominal interest rates will be needed; any need for further change will become apparent in due course. The real mortgage rate of

interest (particularly the post tax real rate) has already fallen to an extremely low level[94].

This view contrasts quite sharply with that of Andrew Britton, who considers only the case of an exchange rate *appreciation*:

Britton Policy measures may also be needed in response to exchange rate movements. If sterling appreciates the opportunity should be taken to cut interest rates again. There is no merit in going back to an over-valued exchange rate, now that the link with the ERM has been broken[95].

However, there were others who saw the benefits of lower interest rates but felt that the relaxation of policy which had already occurred was such that any cut might have to be reversed[96]. Thus:

Currie ... the Chancellor should be cautious about further policy relaxation if he is to avoid a replay of the early 1970s. Interest rates should be cut further in the March Budget only if there is a willingness to raise them again later, possibly quite soon, and only with a firm commitment to bring down public borrowing in the medium term. Further cuts in interest rates now may mean sharper rises later[97].

The problem is that the devaluation may lead to inflation which will eventually undo the competitive advantage gained. However, if interest rates are raised to combat inflation, then the exchange rate will rise and the advantage of the devaluation will be lost anyway. What is needed therefore is a restrictive fiscal policy to keep inflation under control, and low interest rates to keep the exchange rate competitive. Thus:

Sentance Just as a high real exchange rate and high interest rates squeezed investment and exports [in the 1980s], so a lower exchange rate and lower interest rates can achieve the reverse. The shift in policy seen since the middle of last year is therefore in the right direction. However, at the same time, the growth in public and private consumption will need to be contained to release resources for investment and exports and avoid a build-up of inflationary pressures[98].

However, it should be noted that the National Institute's forecast was that the relaxation of monetary policy which had already taken place may be enough to ensure recovery. Thus, for Andrew Britton it was likely that the next move in interest rates will be up rather than down.

Britton If the economy develops as our central forecasts predict, then we must expect increases in interest rates by this time next year [i.e. by Feb. 1994]. The medium term prospects for public sector borrowing, discussed below, may also necessitate tax increases in the December Budget this year - but we need to be sure first that the recovery has actually begun[99].

Summary

This section has outlined the recommendations for monetary policy made by the Panel. It is apparent that there was less consensus on this issue than on the matter of fiscal policy. Although some of the Panel (Congdon, Minford and Davies) favoured a deliberate policy decision to relax monetary policy in order to ensure recovery the rest of the Panel favoured a more wait-and-see approach. Godley thought that a further depreciation in sterling was likely without any further action on the government's part, but Britton thought that action might have to be taken to stop it appreciating.

Conclusions

This chapter has examined the forecasts for the main economic variables made by the Panel of Independent Forecasters in February 1993. Each section has considered one aspect of the forecast and shown how the forecasters thought that part of the economy would develop during 1993. Of course the economy is not made up of discrete, isolated components and the connections between the various parts of the forecasts have been pointed out. In particular, the trade-off between growth and inflation, and the (assumed) links between wages and prices and between inflation (and hence growth) and interest rates have been highlighted

To try and summarise what has been said so far, we can say that the Panel was divided over whether the future for the UK economy was good or bad. The most optimistic was probably Tim Congdon, although Patrick Minford was also fairly optimistic about the longer term future. The reservations both economists expressed about the pace of growth reflected their beliefs, particularly Minford's, that the government would not take the necessary policy decisions to ensure recovery. The rest of the Panel were basically pessimistic. Although Britton, Currie and Davies all forecast growth of between 1.5 and 2.0 per cent it is important to remember that this is still beneath trend, and therefore that the output gap would still be increasing. However, this is not why the group was pessimistic. Like Sentance and Godley, who forecast weak growth in 1993, this group did not believe that the recovery could continue for

long without inflation rising. The reasons for this pessimism varied slightly but centred on either the 'natural rate of unemployment' or the high proportion of consumption in GDP.

We can also note that the Panel differed on how much they emphasised the importance of taking account of particular features of the 1992-93 economic situation, and on how much they were prepared to base their analysis on what had happened in previous devaluations. Britton and Currie, who probably have the largest econometric models, were the ones who paid the most attention to historical precedent. Godley on the other hand was the one who paid most attention to what made 1993 a unique moment in time. How this difference in emphasis maps on to forecasting is difficult to assess. However it does seem as if placing a heavy emphasis on precedent has the effect of increasing the feeling of inevitability about the economic future. Focusing on the here and now, on the contingent, has the opposite effect. Forecasters, like Godley, who emphasise the importance of the present situation are thus more receptive to the idea that the economic outturn is something which can be influenced (just) through appropriate policy action. These distinctions are sometimes rather difficult to spot as they underpin the forecasts rather than feature in them explicitly.

Despite these differences in approach and theory, the Panel did manage to come to a fairly clear consensus when it came to practical action, even if it was, in effect, that the Chancellor should 'wait and see'. In particular it was felt by five out of the seven that there would have to be a significant weakening of the economic situation before a further relaxation of monetary policy would be justified. The other two (Davies and Minford) both argued that the recovery was precarious and that a further cut in interest rates would have been appropriate. Similarly, six out of the seven felt that fiscal policy should not be tightened in 1993, and only Congdon was prepared to advocate tax increases in the March Budget. Thus, although the rest of the Panel believed that recovery was more or less underway, they did not believe the economy was growing sufficiently strongly for taxes to be increased.

The following chapters examine how these forecasts and policy recommendations were modified as the year progressed.

Notes

[1] *Report of the Panel of Independent Forecasters, February 1993 Report*, H.M. Treasury, p.5, para. 5.

[2] op cit. note 1, p.5, para. 5 (bold type in original, italics added).

[3] Andrew Britton, Submission to *Report of the Panel of Independent Forecasters*, February 1993, para 11.

[4] David Currie, Submission to *Report of the Panel of Independent Forecasters*, February 1993, paras 3 and 7.

[5] For example the ESRC held a conference on 'The Future of Macroeconomic Modelling in the UK' in November 1992. See ESRC Macromodelling Bureau Newsletter of February 1993 for a summary. See also: Ormerod, P. (1992) 'Crisis in the Exchange Rate', *Times Higher Education Supplement*, 18 September 1992, pp. 18 & 17; Holly, S. (1991) 'Economic Models and Economic Forecasting: Ptolemaic or Copernican?' *Economic Outlook*, June 1991, pp. 32-38.

[6] Andrew Britton, op cit. note 3, para 1.

[7] Wynne Godley, Submission to *Report of the Panel of Independent Forecasters*, February 1993, para 8.

[8] Wynne Godley, op cit. note 7, para 1.

[9] Wynne Godley, op cit. note 7, para 2

[10] Wynne Godley, op cit. note 7, para 2(a).

[11] Wynne Godley, op cit. note 7, para 9.

[12] Tim Congdon, Submission to *Report of the Panel of Independent Forecasters*, February 1993, para 1.

[13] Tim Congdon, op cit. note 12, para 5.

[14] Patrick Minford, Submission to *Report of the Panel of Independent Forecasters*, February 1993, para 1.

[15] The relationship between unemployment and GDP is more correctly referred to as Okun's Law

[16] Patrick Minford, op cit. note 14, para 2.

[17] Gavyn Davies, Submission to *Report of the Panel of Independent Forecasters*, February 1993, para 2.

[18] Gavyn Davies, op cit. note 17, para 22.

[19] Andrew Sentance, Submission to *Report of the Panel of Independent Forecasters*, February 1993, para 7.

[20] Andrew Sentance, op cit. note 19, para 14.

[21] Andrew Sentance, op cit. note 19, para 19.

[22] Gavyn Davies, op cit. note 17, para 16 (emphasis added).

[23] David Currie, op cit. note 4, para 6.

[24] Gavyn Davies, op cit. note 17, para 19.

[25] Andrew Britton, op cit. note 3, para 6.

[26] Wealth effects are also to be found in strong Keynesian models. See: Arouh, A. (1978). *Empirical Testing and Theory Validation: A Structural Explanation of Persistent Controversy in Economics.* Unpublished PhD, University of Edinburgh

[27] Wynne Godley, op cit. note 7, para 2(b).

[28] Wynne Godley, op cit. note 7, para 2 (e).

[29] Wynne Godley, op cit. note 7, para 3.

[30] Andrew Sentance, op cit. note 19, para 15.

[31] Gavyn Davies, op cit. note 17, para 18.

[32] Davies forecasts that consumers' expenditure will increase by 1.6 per cent, the highest forecast of all the Panel members, whereas Sentance forecasts an increase of just 0.5 per cent, the second lowest.

[33] Tim Congdon, op cit. note 12, para 5.

[34] Patrick Minford, op cit. note 14, para 1.

[35] David Currie, op cit. note 4, para 7.

[36] Andrew Britton, op cit. note 3, paras 3 and 5.

[37] Andrew Sentance, op cit. note 19, para 13.

[38] Wynne Godley, op cit. note 7, para 2(f).

[39] Andrew Sentance, op cit. note 19, para 6.

[40] Andrew Britton, op cit. note 3, para 4.

[41] Although classical economics says that in the end you just get a one-for one increase in prices.

[42] David Currie, op cit. note 4, para 9.

[43] Gavyn Davies, op cit. 17, para 21.

[44] Wynne Godley, op cit. note 7, para 6.

[45] Andrew Sentance, op cit. note 19, para 6.

[46] David Currie, op cit. note 4, para 9.

[47] Andrew Sentance, op cit. note 19, para 19.

[48] See Congdon, T. (1993) 'A Critique of International Micawberism', *Economic Affairs*, November/December 1993, pp 13-18. The same issue also contains a piece from which Godley would probably not dissent: Martin, B (1993) 'Prospects for the British Economy' *Economic Affairs*, November/December 1993, pp. 5-12. For Godley's own analysis see Godley, W. 'Britain's Economic Prospects: No Recovery in Sight?', *The State of the Economy 1993*, IEA Readings No. 39, London: Institute of Economic Affairs.

[49] Tim Congdon, op cit. note 12, para 11.

[50] Tim Congdon, op cit. note 12, para 2.

[51] Tim Congdon, op cit. note 12, para 5.

[52] Wynne Godley, op cit. note 7, para 5.

[53] David Currie, op cit. note 4, para 8.

[54] David Currie, op cit. note 4, para 8.

[55] Andrew Britton, op cit. note 3, para 7.

[56] Andrew Sentance, op cit. note 19, para 11.

[57] Gavyn Davies, op cit. note 17, para 20.

[58] David Currie, op cit. note 4, para 10.

[59] Andrew Britton, op cit. note 3, para 9.

[60] Gavyn Davies, op cit. note 17, para 19.

[61] Andrew Sentance, op cit. note 19, para 11.

[62] This point was made clear in interviews conducted with Professor Congdon:

> 'In my view the government does not have and should not have an obligation to provide full employment whatever that might mean, and it doesn't have any obligation or even an objective to maximise living standards - these are things that are done by individuals themselves, subject to their own abilities and so on, they are not part of what the government exists to do or what the state exists to do.'

Source: Tim Congdon, Interview, 2 June 1994, pp. 2-3.

[63] Wynne Godley, op cit. note 7, para 4.

[64] Patrick Minford, op cit. note 14, para 1.

[65] Patrick Minford, op cit. note 14, para 11.

[66] Patrick Minford, op cit. note 14, para 5.

[67] Patrick Minford, op cit. note 14, para 8.

[68] Patrick Minford, op cit. note 14, para 9.

[69] Patrick Minford, op cit. note 14, para 10.

[70] Gavyn Davies, op cit. note 17, para 24.

[71] David Currie, op cit. note 4, para 19.

[72] David Currie, op cit. note 4, para 18.

[73] Andrew Sentance, op cit. note 19, para 10 (emphasis added).

[74] Patrick Minford, op cit. note 14, para 13.

[75] David Currie, op cit. note 4, para 17.

[76] Wynne Godley, op cit. note 7, para 12.

[77] Wynne Godley, op cit. note 7, para 10.

[78] Wynne Godley, op cit. note 7, para 11.

[79] Wynne Godley, interview, 9 June 1994

[80] David Currie, op cit. note 4, para 22.

[81] Tim Congdon, op cit. note 12, para 10.

[82] Gavyn Davies, op cit. note 17, para 5.

[83] Andrew Sentance, op cit. note 19, paras 22 and 23.

[84] Wynne Godley, op cit. note 7, para 14(3).

[85] Wynne Godley, op cit. note 7, para 18, (emphasis in original)

[86] Andrew Sentance, op cit. note 19, para 21.

[87] Andrew Britton, op cit. note 3, para 12.

[88] Wynne Godley, op cit. note 7, para 19.

[89] Andrew Britton, interview, 1 March 1993

[90] Tim Congdon, op cit. note 12, para 8.

[91] Patrick Minford, op cit. note 14, paras 20 and 21

[92] Gavyn Davies, op cit. note 17, para 2.

[93] Andrew Sentance, op cit. note 19, para 24.

[94] Wynne Godley, op cit. note 7, para 15.

[95] Andrew Britton, op cit. note 3, para 15.

[96] As an aside, it is interesting to note the reluctance of authorities to adjust interest rates - the exchange rate moves constantly with no controversy; why are small adjustments to the interest rate so important?

[97] David Currie, op cit. note 4, para 4.

[98] Andrew Sentance, op cit. note 19, para 20.

[99] Andrew Britton, op cit. note 3, para 13.

Chapter 6

This chapter examines three main themes. The first of these is the forecasts produced by the Panel of Independent Forecasters in July 1993. The chapter therefore begins by discussing the changes which have been made to the forecasts since February (i.e. the ways in which things have not turned out quite as expected) and the differences which remain between the forecasters despite the fact that 1993 is now half over. This part of the chapter thus further explores the variability of the economic data and the contingency of economic forecasting.

The second theme discussed is the explanations given for the falls in unemployment which occurred during the early months of 1993; the third is the extent to which the PSBR is structural or cyclical (i.e. the extent to which it is a problem for policy or something which will correct itself). The analysis of these two debates is used to highlight a pair of related issues. The case of unemployment shows how economic forecasters must rely on all sorts of non-econometric theories and evidence in order to explain important parts of the economy. The case of the PSBR's sustainability shows how the same data set can support radically different interpretations of economic events. The chapter begins by examining the changes which have occurred since February.

July 1993 Report

Introduction

In July 1993 the Panel of Independent Forecasters met for the second time. Since 4 months had passed since their first meeting in February, and 1993 was now half over, the July meeting gave the forecasters a chance to discuss how well their original assessments were matching up to the unfolding economic events. The first point they make is that, since February, it has become clear that the recovery in the UK economy is now underway, and that positive GDP growth has now officially been recorded. In addition, inflation has not risen as much as some had expected and unemployment, rather surprisingly, has fallen:

> When we last met in February most of felt that there were prospects of at least a modest recovery in 1993. But we had little in the way of firm evidence to support that view. Since then non-oil GDP has recorded its first significant rise in three years, manufacturing output has risen strongly and

> there have been four consecutive monthly falls in unemployment. These are welcome figures. Nevertheless, the pace of recovery remains uncertain and **growth** will be held back this year by the downturn else where in Europe. The likelihood of **inflation** exceeding the Government's target range in the short term has receded somewhat, though for most of us it is still well within the realms of possibility. And while we do not know quite what to make of recent labour market statistics, we all believe that, in the short term at least, **unemployment** has either passed its peak or is close to it[1].

Taken together these figures imply a rather better outturn for the first half of the year than was generally expected in February. However, despite their increasing certainty that some sort of recovery has begun, some of the Panel remain concerned about its robustness. This uncertainty about the strength of the recovery is reflected in their policy recommendations. One the one hand, the recovery is sufficiently on course for further increases in public spending to be unnecessary. On the other hand, policy should not be tightened just yet:

> Although we do not agree on specifics, we are clear that fiscal policy should not be loosened - and for most of us a further tightening will at some stage be required. We also caution against a tightening in monetary policy at present, indeed in current circumstances if fiscal policy were to be tightened further than already announced, we would all recommend a loosening in monetary policy[2].

The recommendations are thus that the overall mix of fiscal and monetary policy should not be changed at present. If however, fiscal policy were to be tightened then interest rates should be cut in order to offset its effects on growth. The maxim at work seems to be 'When in doubt, do nowt'. The following sections outline the views of each of the forecasters in more detail. However, for brevity and to avoid undue repetition, the emphasis will be on forecasts for demand and inflation and, in particular, the changes made since February. Where no mention is made of a particular forecaster or forecast, the reason is that his position has not changed significantly from that outlined in the previous chapter.

GDP Growth and Demand

One of the things which is most noticeable about the July forecasts, as compared to the February ones, is that they are much more optimistic about the recovery in GDP. For example, forecasts for GDP growth for 1993 made in February ranged from 0.2 per cent

to 2.0 per cent, with an average of 1.1 per cent. In July, the forecasts ranged from 1.25 per cent to 2.0 per cent, the average being 1.5 per cent. The range of outcomes has thus halved in the 4 months since February and the average has also shifted upwards, reflecting the increased certainty that recovery is under way. Thus:

> The rise in **GDP** in the first quarter was a bit stronger than most of us were expecting when we last met. While those of us who were anticipating weaker first quarter have revised up our forecasts for 1993 as a whole, the outturn has not affected our views on the pace of forecasts from now on. Our forecasts for growth in 1993 range from 1.25 to 2 per cent with an average of 1.5 per cent. Apart from Godley we all see growth prospects improving significantly in 1994[3].

This increase in optimism is a feature of all the forecasts, the sole exception being Andrew Britton who, as discussed in Chapter 5, was already more optimistic than the average. For Britton, things were turning out pretty much as expected - the domestic recovery was slightly stronger, the European recession slightly weaker but no significant changes were felt to be necessary:

> *Britton* The evidence of a recovery in economic activity is very much in line with our forecasts since "Black Wednesday". We have therefore left our growth forecast for this year unchanged at 2.0 per cent. We have therefore [sic] revised up our forecast of domestic demand a little, but this is offset by a rather less optimistic view of exports as activity in Europe weakens.[4]

For the other forecasters, however, it is clear that the growth in the economy, particularly the domestic economy, was stronger than they were expecting, mainly as a result of the change in policy since September 1992. Thus, for example, Tim Congdon writes:

> *Congdon* Domestic demand has also responded positively to the change in economic policy since sterling's departure form the European exchange rate mechanism on 16th September last year. The drop in the clearing banks' base rates from 10 per cent to 6 per cent has stimulated worthwhile increases in mortgage commitments, sales of consumer durables and car registrations, which are good leading indicators of demand. The increase in exports and higher spending on such big-ticket items as houses and cars have already turned output around.[5]

As a result of these 'items of news', Congdon raised his forecast of GDP growth from 1.1 per cent to 1.5 per cent. Although his colleagues also increased their forecasts for GDP growth, many did so with reservations that Congdon did not appear to express. For example, although David Currie recognised that the increase in domestic demand was better than expected, this good news was tempered by other factors, particularly the deepening of the recession in Europe. Thus:

> *Currie* Our view about growth prospects is broadly similar to that which we took in February. Although the world, and especially the European, outlook now appears even weaker than in February, so that the prospect for exports is less favourable, domestic spending appears correspondingly more robust. In consequence, our forecast for growth is at around 1.5 per cent this year and 2.75 per cent in 1994[6].

The forecast for 1.5 per cent GDP growth is just 0.1 per cent more than the February forecast and, interestingly, is exactly the same as Congdon's. However, this numerical similarity at the aggregate level is quite misleading, as the forecasts are actually based on very different analyses of the economy. Congdon sees the growth as being more or less equally driven by domestic demand and net trade - domestic demand increases GDP by 0.8 per cent, net trade by 0.7 per cent. Currie on the other hand believes that the *only* source of growth in the economy will be domestic demand, which will increase by 2.4 per cent, and that net trade will actually reduce GDP by 0.8 per cent. In other words, despite starting from the same data and reaching the same aggregate forecasts, the kinds of recovery being predicted by Currie and Congdon are very different.

In fact, Currie and Congdon represent the two extreme views on the composition of the recovery, with the rest of the Panel somewhere in-between and nobody forecasting an entirely export-led recovery. Given the theoretical orientations outlined in Chapters 1 and 4, the positions of the other forecasters relative to Congdon and Currie are not hard to predict. The closest forecast to Congdon's is that of fellow monetarist Patrick Minford, whose forecast for GDP growth of 1.5 per cent (i.e. the same as Congdon's and Currie's) includes a positive contribution from net trade of 0.6 per cent.

Closest to Currie, in terms of the composition of his forecasts, is Gavyn Davies whose forecast is the only one, other than Currie's, which predicts that net trade will reduce GDP. The rest of the Panel are somewhere in the middle. Britton and Godley project that net trade will contribute nothing to GDP growth and that any increase in

activity will come solely from domestic demand. Sentance, on the other hand, projects a small positive contribution to GDP from net trade and is thus slightly closer to the Congdon-Minford pole than the rest of his mainstream colleagues.

In addition to the variation in the forecasts for the composition of the growth in aggregate demand (i.e. the split between net trade and domestic demand), each of these two components was also forecast differently by different members of the Panel. For example, when discussing net trade the Panel write:

> While on average we expect no contribution to GDP growth from **net trade** in either 1993 or 1994, this conceals a wide range of views. For 1993 we all expect UK exporters to do relatively well against a weak world background, but we disagree on import growth, where our forecasts range from 2.25 to 7 per cent. In part this reflects different projections of domestic demand but while we all expect a rise in import penetration, we differ as to its extent[7].

Because the difference in net trade is, in part, due to the differences in forecasts for domestic demand, these will be examined in slightly more detail first. One interesting thing to note, and which is a change from the situation in February, is that the forecasts for consumers' expenditure (an important part of domestic demand) are now much more consensual, averaging an increase of around 1.5 per cent during 1993[8]. In each case, as in February, the forecast is the difference between two opposing effects. However, unlike in February, in each case the judgement is now fairly similar[9]. Thus:

> Our forecasts for **consumers' expenditure** are broadly similar and are each informed by much the same considerations: on the one hand the lagged effect of the easing of monetary policy since last September; on the other slow growth of personal incomes and the high level of personal sector debt. Falls in the savings ratio over the next two years are features of all our forecasts[10].

The variation in their forecasts for domestic demand thus relate almost entirely to their forecasts for investment which, as the Panel point out, vary widely:

> Congdon, Godley and Sentance expect a continuing poor construction performance, particularly in light of the oversupply of commercial property, and a fall in North Sea investment. The rest of us are more optimistic, expecting high profitability to encourage investment despite continuing spare capacity[11].

The interesting thing about this bifurcation of investment forecasts is that all parties acknowledge the same influences - it is just that the weighting in the final mix is different. For example, the investment optimists (Andrew Britton, David Currie and Gavyn Davies) stress the manufacturing part of the total investment picture, but discount the construction part, and thus come to a fairly positive conclusion. The pessimists, on the other hand, minimise the importance of manufacturing and emphasise the weak state of the construction industry. Both positions are illustrated in the quotations below.

The 'investment optimists' make their case as follows:

Britton ... we really are saying that the surveys have been too pessimistic and that when they filled those [CBI] surveys in, the firms didn't realise the extent of the recovery that was coming along. I suppose basically it reinforces our optimism about the economy as a whole and that with a relatively buoyant outlook for exports and certainly much better profits this year, with that goes the expectation of more company investment ... I think that the construction sector has been particularly hard hit in the recession and that there was over-investment in the 80s, particularly concentrated in commercial property, so that it may turn out to be the sector which lags behind ... [but] I don't think that the problem with commercial property need hold back the other sectors.[12]

Currie Maybe we are too optimistic, but we do see the corporate sector as largely retrenched ... Profitability is not bad, and there is the prospect of growth. And against that background, and against the background of really quite intense international competition, we think companies will be investing. Now the objection to that of course is that they have got lots of spare capacity hanging over them from the past. I mean that is fine, except that quite a lot of investment may be done by different firms. Companies use new investment as a strategic weapon, and so you can get investment even when there is a significant element of spare capacity, and it is on those sort of views that we have based our forecasts ... Construction is in a bad way ... [but] there is also the question of how you factor [it] in ...The question is, is it going to go back to the old capital-output ratio, or have these sectors become more capital intensive, [and] therefore, will they be, in steady state, investing more than they were in the past? We've got an element of that second story in there, so the process levels out a bit earlier than it might otherwise have done.[13]

On the other hand, the investment pessimists make their case as follows:

> *Sentance* I think those of us who expect a weak investment growth, are looking at it from a point of view of building up the investment picture. I mean it is tempting to say that manufacturing is more competitive, manufacturing should be encouraged to invest as a result of that as the economy grows and so on, but manufacturing is only 20 per cent of the total investment picture, if we're talking about total fixed investment. Now in terms of the total fixed investment, we have a large part in the construction sector, the property sector, [and] there is a massive overhang in property, which is going to dampen down that sector ... So although we expected some recovery in investment, we expected it to be muted[14].

Thus, the reasons given are mirror images of one another. For the optimists, manufacturing dominates construction, for pessimists, construction dominates manufacturing. Finally, it is worth comparing the views of Andrew Britton and Andrew Sentance on the importance of survey data. Britton it will be recalled, believed that survey returns were inaccurate guides to investment because firms had underestimated the strength of the recovery.[15] Andrew Sentance (who works for the CBI) however took a rather different view. When asked why he did not think that the increasing confidence of manufacturing firms would be enough to turn the total investment total around, it was survey evidence which he cited:

> *Sentance* But you are going to have to get a very big increase in manufacturing investment, but we are not seeing them in our surveys yet ... The last time we did a survey of investment intentions in April, there was still more manufacturers planning to cut investment than increase it. So I would want to see more evidence from our surveys to convince me that investment was about to rise.[16]

Contrast this with David Currie:

> *Currie* Even though surveys suggest that companies are at present operating below capacity, our view is that new investment will be profitable and will go ahead on this basis. We are forecasting a rise in whole economy investment next year of as much as 7 per cent[17].

Leading indicator models and new data

Before moving on to the forecasts for inflation, there is one other point which comes out of the changes made to the forecasts for demand between February and July, and this is

the effect of new data. The example given below is taken from Gavyn Davies's submission, and focuses on the use of leading indicator VAR models which are used by Goldman Sachs as a check on other forecasting methods.

As pointed out in Chapter 5, Gavyn Davies was initially one of the more pessimistic forecasters on the Panel. Although his February forecast for GDP growth was 1.5 per cent, and therefore above the average, it was based an assumed reduction in base rates to 4.5 per cent by the end of the year. Without this change in policy, 'the short and medium term outlook for the economy would be a good deal more pessimistic'[18]. In July his forecast was for GDP growth of 1.7 per cent. Superficially then very little has changed, with the forecast increasing just by 0.2 per cent. However, a closer examination reveals that several very significant changes have been made, as will be shown below.

Firstly, as was the case for many of his colleagues in July, Davies had to concede that 'the recovery in demand in the early part of this year ha[d] been stronger than expected'[19]. However, this is not what is particularly interesting about Gavyn Davies's forecasts. Rather what is interesting is his open and frank discussion about the use of leading indicator models in forecasting. Many other forecasters also use leading indicator models in their work and so the experiences described are no doubt familiar to several of his colleagues.

A leading indicator model is basically a regression model of data series which have, in the past, tended to precede the economic cycle. The actual list of variables used varies between forecasters and may be determined by theory or by empiricism. In his submission, Tim Congdon identified 'mortgage commitments, sales of consumer durables and car registrations' as 'good leading indicators of demand'[20]. The leading indicator model used by Gavyn Davies draws on a different set of variables but the principle is similar. The following discussion examines the interplay of leading indicator models, econometrics and judgement in the forecasts produced in July.

It has already been pointed out that Davies's forecast for July was numerically slightly more optimistic than his February forecast. The question is, what are the sources of the extra growth? Like everyone else, Davies is attentive to the recent statistics. Thus he notes that:

> *Davies* When my last brief for the Panel was prepared on 12 February, it appeared that output in the economy had stabilised, but there was still no unequivocal

sign that a recovery had started. It now appears more certain that the trough of the downturn was reached in early 1992, initially followed by a period of flat activity. In the last four months, the balance of evidence has suggested that a clear recovery has now started, and that it proceeded at a rapid pace in the early part of this year. In the latest 3 months, manufacturing output has risen by 2.2 per cent on the previous quarter, and retail sales volume is up by 0.8 per cent. This evidence of strong growth has substantiated the earlier evidence of business surveys, which had turned up late last year. Furthermore, there have been definite signs of improvement in both the labour and housing markets, which had previously acted as severe constraints on consumer sentiment[21].

It is interesting to note that the surveys which now reinforce Davies's optimism are the ones he discounted as 'unreliable' in his February Submission (see Chapter 5). In addition to the new data, another influence on Gavyn Davies's forecasts is a leading indicator model. The model, which is a VAR model incorporating, amongst other things, business optimism, the exchange rate and interest rates, is used by Davies as a sort of consistency check on his forecasts. What the leading indicator model provides is a straightforward extrapolation based on past data. Although its output is not accepted uncritically, forecasts which are significantly different from what would be expected based on past experience call for greater thought and explanation. Thus

Davies ... the main forecasting system has broken down at times in the past and if I get very, very different forecasts from what is essentially just a statistical forecasting framework, then I think about what I want to show in the main forecast ... [I]f I deviate a lot from a statistical forecast, from a VAR forecast, I want to know why.[22]

It is in this sense that the VAR model provides a consistency check.

The interesting thing to note about the VAR model in July is that it is much more optimistic than it was in February. As Gavyn Davies explains:

Davies At the time of the Panel's last report th[e VAR model] suggested that GDP growth in calendar 1993 could be around 1.5 per cent, accelerating to 3 per cent in 1994, *assuming that base rates were cut to 4.5 per cent* by the end of this year ... The model now predicts that GDP will grow by 4.4 per cent from 1993Q2 to 1994Q2 *on the assumption that base rates rise to 7 per cent* by mid 1994[23].

The significant point here is not simply that the forecast has doubled in 4 months, but rather the fact that it has increased at all given the change in assumptions which have been made. In February, the model was forecasting beneath trend growth in an increasingly relaxed policy environment - in particular interest rates were assumed to fall by 2 per cent. In contrast, the July forecast, which is for above trend growth, takes place in an increasingly deflationary policy environment which would, all other things being equal, be expected to reduce the forecast for GDP.

In other words, in the VAR model 'favourable moves in several crucial variables, including business optimism, the corporate default spread, and lagged values of the real exchange rate, short term interest rates and the reverse dividend gap'[24] mean, in the space of 4 months, that the setting of policy could change from being reflationary to being deflationary. With models as sensitive to the data as this it is no wonder that they are interpreted with considerable caution and, despite the model, Davies's optimism remains tempered:

> *Davies* The recovery in demand in the early part of this year has been stronger than expected, and leading indicator models suggest that GDP growth in the next 12-18 months might be at an annualised rate of over 3 per cent ... However, personal debt ratios remain high and consumers remain extremely resistant to any signs of higher inflation. The stop / start pattern which has been observed in the US recovery may happen here too, but it would be quite surprising, on current policy settings, if the recovery petered out altogether[25].

Similarly, Davies remains unconvinced that his February policy recommendations were inappropriate:

> *Davies* As noted above, it is not clear if leading indicator models based on past behaviour make sufficient allowance for the impact of the fiscal tightening, or indeed for the continuing overhang of personal sector debt. As US experience clearly demonstrates, it is possible that the consumer might be subject to setback as the growth in real disposable income begins to slow, as the growth in real personal disposable income begins to slow, and reliance is placed on a decline in the savings ratio. This adds to the case for interest rates to remain low over the next two years[26].

Inflation

Unlike the forecasts for GDP growth, the forecasts for inflation made in July were virtually unchanged from those made in February. In February the forecasts for underlying inflation at the end of 1993 ranged from 1.0 to 4.9 per cent (average = 3.5 per cent); in July the range was 1.0 to 4.1 per cent (average = 3.3 per cent). Nevertheless, the forecasts were generally a bit lower (with the exception of Congdon, whose forecast was already lowest) and so the Panel could write:

> Prospects for **inflation** in the very short term have improved since February. Now only Britton and Currie forecast that underlying RPI inflation will exceed 4 per cent in the next year, though this outcome would be within the margins of error for most of the rest of us. But we agree that any breach of the Government's 1-4 per cent target range will be small.[27]

However, there is an important sense in which the unchanged inflation forecasts do represent a very important change. In Chapter 5, the idea of a growth-inflation trade off was introduced, the idea being that the benefit of extra growth comes with the cost of extra inflation. However, in this case we appear to be having the benefit of extra growth (remember that the GDP forecasts had generally been revised up) without the cost of any additional inflation (if anything the inflation was even less that expected). What this points to then is not a change in the inflation forecast, but a change in the growth-inflation trade off - a change in one of the most fundamental features of the economy.

Given that the growth-inflation trade off is not the same as it used to be, the question now is: 'What has changed?' It is clear that prices have not risen as much as expected but, because a price index contains many different prices, it is still important to know which prices have stayed particularly low, thus keeping the increase in the overall index down. One possible explanation is that wages have not risen as much as might have been expected. In Chapter 5, it was shown that the inflation pessimists drew clear lessons from past experience, in particular the tendency for UK wages to rise following devaluation, and thus cancel out the competitive advantage gained from the lower exchange rate. Given that something in the economy has changed, might wage behaviour be the thing?

From David Currie and Andrew Sentance's submissions it is clear the rate of increase in wages has remained very low and that this has helped to counter the inflationary effect of the devaluations:

Currie While the devaluation of the pound has had an immediate impact on import prices and on producer input prices, the effect on final prices has been offset by falling wage settlements and a decline in unit labour costs in manufacturing.[28]

Sentance Though higher import prices following sterling's depreciation last year and the planned increases in indirect taxation will exert some upward pressure on inflation, this is expected to be offset by the downward trend in unit labour costs.[29]

Gavyn Davies makes a similar point when he writes:

Davies Inflation pressures have remained extremely subdued, with labour cost inflation in the whole economy falling to 25 year lows. This has almost entirely offset the early effects of the sterling devaluation on domestic prices.[30]

However, despite the unusually low levels of wage inflation highlighted by all the economists, this exceptional state of affairs is not unexpected and does not, therefore, represent a break with the past relationships:

Davies The Goldman model of the wages and prices sector of the economy has not broken down during the recession, which suggests that there is nothing inexplicable in the recent decline in labour cost inflation, given the behaviour of the rest of the economy.[31]

Similarly, Andrew Britton, another inflation pessimist writes:

Britton Wage settlements remain low, but not lower than one would expect given the slack in the labour market and the headline figures for the RPI.[32]

The answer to the question what has changed is thus to be found elsewhere. However, there is no clear consensus as to why inflation is lower than the pessimists expected and, interestingly, most of the Panel would say that nothing much has changed at all. The reason is that although the rise in wage inflation has not happened *yet* it is still expected sooner or later.

Britton There remains a serious risk that underlying inflation will rise to over 4 per cent by the end of this year or early next year. With unemployment no longer rising, and with profits increasing sharply, wage claims are very likely to increase. The rise in import prices still implies higher prices sooner or later for a wide range of goods and services. But the more

protracted the adjustment the less the effect on inflation, at least in the short term[33].

Currie Underlying inflation (RPI exc. mips) should be within the target range this year but for much of next year we see it in the 4-5 per cent range, disappointing to the Government but a reasonable post-devaluation outturn. The critical variable is the earnings response - which is the cause of our pessimism. We expect earnings growth to pick up from 4 per cent this year to 6 per cent in the more buoyant conditions of 1994.[34]

In other words, there is nothing particularly troubling for the inflation pessimists about the lower than expected inflation figures. In particular, they do not force a re-evaluation of the econometric relationships involved, nor do they force a significant reassessment of the longer term outlook. The significance of the July report is that it gives the Panel an opportunity to compare events with their expectations. We have already seen that the pessimists interpret the figures as being consistent with their expectations, now it is the turn of the optimists.

For the inflation optimists the low inflation figures simply vindicate their theory. Of course, they also indicate that the pessimists are wrong, particularly about the longer term trend for inflation, which they said will be up towards 5 per cent. The optimists, who it should be remembered are in the minority, are forecasting that inflation will get lower and stay low for several years. However, it is worth looking at why the optimists think that inflation will be low, as their reasons are very different. For Congdon, it is a matter of theory - he has the right one; the others don't. Thus he writes:

Congdon What is the crux of the difference between the consensus view [i.e. that inflation will on an upward trend from mid 1993, reaching 5 per cent by end-1994] and the position taken here? The answer is that the majority of economists believe that above-trend growth of output causes an acceleration in inflation, whereas our view relies on the idea that the level of output (relative to its trend level) is the dominant determinant of the direction of inflation. The relative importance of the rate of change and or the level of output (and unemployment) in the inflationary process is an empirical matter which can be assessed by looking at past data. Lombard Street Research's Submission to the Panel in February included an equation, based on data since 1963, in which the level of output, not the rate of change, was a powerful influence on inflation. Further work has not required any significant reassessment[35].

For Minford, the other inflation optimist, additional factors are at work. Like Congdon, he believes that the unemployment and spare capacity produced by the recession will have an effect. However, this is only a 'temporary effect lasting as long as the recession itself'. [36] The other factor which influences inflation, particularly over the longer term is inflation expectations:

> *Minford* If people expect inflation this very fact produces wage demands and price rises which then produce inflation on cue. Inflation is a self-sustaining process through this mechanism. Once embedded in an economy it continues, assuming no recession interrupts it, of its own momentum. Hence expectations are the cause of inflation, whereas boom and slump merely have temporary effects on it[37].

The principal cause of Minford's optimism is thus his belief that attitudes towards inflation in the UK economy have changed significantly in recent years. Somewhat sociologically, and rather uneconometrically, Minford interprets recent events as indicating a change in norms and values. Of particular significance in Minford's story is the Conservative party's victory at the previous general election which he attributes to this change in popular opinion. In a section of his submission entitled 'The Death of Inflation', Minford writes

> *Minford* The Tories were re-elected [because they had been tough on inflation and prudent on public finances[38]], defying (apparently logical) predictions of a hung Parliament or worse. I believe we have evidence here of a new attitude to inflation among the British floating voters, no doubt dating from the late 1970s. Even now, astonishingly considering the seriousness of the recession, one finds that advice to cut interest rates is often opposed in popular discussion by those worrying about re-igniting inflation. That too suggests a quite new concern about inflation. Discussions in the 1970s were not in this mould[39].

In summary then, the unchanging inflation forecasts seemed to imply two possibilities. The first was that the growth-inflation trade-off in the economy had changed and that more growth was now possible for a given amount of inflation. If true then this would represent an important development in macroeconomic reality. The other possibility was that nothing much had changed between February and July. The position taken by the Panel of Forecasters was, almost universally, the latter, that nothing had changed - the lower inflation figures were a temporary phenomenon, the result of firms having to

keep prices down because of the depressed state of both the world and domestic markets. Thus the pessimists stuck to their original stories and, as a consequence, remained doubtful of the economy's ability to grow significantly before inflation became a problem once again.

For the optimists things were different. For Congdon it was pretty much business as usual, as evidenced by his reference to an econometric equation. Of course, Congdon interpreted events as supporting his view that inflation was on a downward trend rather than an upward trend, but nevertheless, nowhere does he mention that anything fundamentally new is happening in the economy. Minford on the other hand does see the low inflation figures as portending something rather special. The point is not that Minford, on seeing the inflation numbers, suddenly realised that something new was afoot. He did not. Rather the point is that, in interpreting the numbers, Minford sees support for a position which does recognise that major changes have occurred.

> *Minford* Whether the extraordinary wage figures of today simply mirror sharp recession we will not know for sure until the recession is over and they have stayed down on a new lower plateau. But my suggestion here is that they are taking on board the new political realities as they concern inflation. I note that there is little demand for stimulative policies. This cannot have escaped firms and workers engaged in wage bargaining. We have had a sea-change brought about by a huge deflation whose effects have basically been accepted by popular opinion.[40]

The potential for social change to undermine econometric models was discussed in detail in Chapter 4 and here we have an example of how difficult that judgement can be to make. In July 1993, growth was higher and inflation simultaneously lower than was expected. Does this imply that something in the underlying relationships had changed. For six of the seven it did not, for one it did. We shall have to wait until Chapter 8 and the final outturns to see who, if any, will admit to error.

Unemployment

As was discussed in the Chapter 5, all the Panel expected unemployment to remain at its current level for the whole of 1993, and for the majority, a further increase in the number of people out of work was likely. However, in early 1993 unemployment fell quite dramatically for four months running (for example, in both February and March the claimant count fell by 26,000). Both the timing and the scale of the falls were

completely unexpected and, as such, they can be seen as something of an 'anomaly'. Because of this, their interpretation was far from straightforward.

The problem faced by the forecasters was how to relate the falls in unemployment to what was happening in the rest of the economy. In other words, what did the unexpected decrease in the number of people out of work imply for the future? Had the UK economy come out of recession more quickly than was expected or were they a statistical aberration? Would the falls continue or would unemployment rise again? The difficulty of the situation is reflected in Gavyn Davies's submission to the Panel:

> *Davies* Considerable uncertainty surrounds the immediate outlook for unemployment. The declines of 26,000 per month in February and March may have been partly due to seasonal adjustment problems, but there have been some confirmatory signals of an improving labour market in other information, including business surveys, and the further fall of 26,000 in May is obviously significant.[41]

By examining the ways in which these unemployment figures are explained I will make two points. The first is to illustrate the range of interpretations of the unemployment situation on offer at that time. The second is to illustrate how, as a result of this inconclusiveness, economic forecasters must rely on all sorts of non-econometric theories and evidence in order to explain important parts of the economy. I begin by outlining the basic theories of unemployment used by the Panel members.

The Mainstream View

Amongst the Panel of Forecasters, and perhaps amongst economists more generally, there are two basic stories about unemployment. One remains true to the tradition of the Classical economists and focuses on the adjustment of wages, which is in turn influenced by institutional factors such as the power of trade unions, labour mobility and the benefit system. The other theory, which is of a much more recent origin, supplements this story with an additional factor known as 'hysteresis'. The main distinction therefore is between those who believe in the idea of 'hysteresis' and those who do not.

Although the relative newcomer, it is the hysteresis model, developed by Professors Steve Nickell and Richard Layard[42], which is the mainstream representation of the labour market. It is often referred to as the 'Layard-Nickell' model. The theory is

based on the observation that after there has been a recession, unemployment does not fall back to the level it was before, but appears to remain 'stuck' at a new higher level. The basic ideas behind the model can be summed up as follows:

> *Britton* If you look at the course of unemployment, it goes up in steps, and it never falls back to the extent it did before. This is perhaps due to the problems in the labour market or possibly the economy more generally. Hysteresis may not simply be a matter of people losing their ability, or their acceptability, or something in the labour force. But it may also be to do with the loss of opportunities for innovation, loss of investment and so on. So that you can't simply assume that the capacity is in some sense 'there' to make up after the recession.[43]

The idea of hysteresis is used slightly differently by the different members of the Panel, but the majority (Britton, Currie, Davies and Sentance) would all subscribe to something like the following ideas, expressed by Andrew Sentance:

> *Sentance* Basically, my line of argument is that the genuine natural rate is probably 1 to 1.5 million ... but overlaid on that you have this hysteresis effect which makes it appear that you can't get unemployment down below 2 to 2.5 million without inflation picking up.[44]

This situation arises because what hysteresis does, in effect, is reduce the available supply of employable people to a subset of the total number of unemployed. There are a variety of reasons why the unemployed might become unemployable, most of which are nothing to do with economics. Nevertheless, if a number of people are excluded from the labour force then the supply of available labour is reduced. It is basic economics that if the supply of something is restricted then its price will rise. However, it is equally basic to economics that a rise in the price of something will simultaneously act to reduce demand for it. What hysteresis does therefore is to create a new equilibrium in the labour market at which the price of labour will be higher, and the demand for labour lower. Of course, the unemployable labour force do not go away[45], and so another effect of hysteresis is that the natural rate of unemployment, (i.e. the rate at which wages and prices are stable) appears to be raised. In other words, for the majority of the Panel, the total number of people unemployed is made up of two components. There are those who remain out of work because of the institutional and other features of the economy (i.e. the 'natural rate') and those who remain out of work

simply because they are unemployed. These causes are quite separate and require different sorts of policies to tackle them, as is made clear by Andrew Sentance:

> *Sentance* [It is important] not to confuse a sort of long run natural rate of unemployment with the effect of this hysteresis. I think the hysteresis effect can be tackled with the right labour policies and sensible macro management. The long-term natural rate may be the product of fairly deep structural things, the bargaining structure, union membership and so on, which can't readily be changed by economic policy and may be very difficult to shift altogether.

To sum up, the idea of hysteresis is important for macroeconomic policy because it implies that being unemployed, particularly long-term unemployed, effectively renders people unemploy*able* for the future unless special training programs are created. Given the implications of perpetual unemployment for the public finances (not to mention the lives of those consigned to life on benefits) economists who accept the idea of hysteresis tend to favour fairly active labour market policies. The following sentiments are more or less typical:

> *Britton* My interest [in unemployment] has been involved by wider social considerations - what is wrong with the state of the nation seems to me to have a lot to do with a sustained period of high unemployment, so that my interest in trying to think how full employment could be restored is not just a question of trying to make the economy grow faster and have more real wealth to distribute, but also feeling that socially it is divisive to have so many people unemployed and that they ought to be able to have a role to contribute to society beyond simply claiming their dole. Not just so many mouths to feed, but actually somebody with a useful contribution to make.[46]

> *Currie* First of all, you don't [reduce unemployment] through a big, rapid boom in demand because that will hit the buffers and you will jack up inflation. I think a steady increase in demand may well be helpful, because if there are people who are long-term unemployed, I mean you have these hysteresis-type effects in the labour market, you have a greater chance of correcting those against a background of steady growth. So maintaining a reasonable steady rate of growth in demand would be sensible. But I think it also requires fairly active measures, putting emphasis on labour mobility, putting more emphasis on training and skills, and possibly even with the tax scheme

that Lamont introduced experimentally in the Budget [in which you can] use part of your benefit as a subsidy for your wage for a period of time.[47]

Sentance [You would need] things like better training programs for the long-term unemployed. I would actually have an employment program for the long-term unemployed and link this very closely into the benefit system. You know, a report of this type doesn't really give you [a] chance to expand all your ideas on this. These are my personal ideas, I'm not sure of the extent to which they are robust, but my notion is that you have to break into this cycle where people apparently appear to drop out of the labour market and come to exist on benefits for a long period of time ... [I think you need to] have a set of macroeconomic policies which should enable you to have above trend growth and that you then have a sort of supply-push side policy in the labour market whereby you have active training for the long-term unemployed.[48]

The Classical View

The alternative to the mainstream account is that of the Classical economists. This is the basis of the approach favoured by Minford and Congdon. According to this view, hysteresis simply does not exist and the high levels of unemployment seen in the UK are the result of policy mistakes. However, as there are some differences between the importance which Minford and Congdon attach to the labour market, they are discussed separately.

Congdon's theory of the labour market is fairly simple and reflects his own personal and political priorities. As noted in Chapter 5, it is Congdon's belief that the responsibilities of government do not include the maintenance of 'full employment'. Thus, Congdon seems to think about the labour market only to the extent that it affects his forecasts for other variables. In this analysis he relies heavily on the concept of the 'natural rate' (defined, as before, as the rate of unemployment at which prices would be stable).

Like the most of the rest of Panel, Congdon believes that, at 2.9 million, unemployment in the UK was above the natural or equilibrium rate[49]. From this it follows that the competition for jobs must be strong and that the pressure on wages, and hence on prices, is correspondingly weak. This is all perfectly consistent with basic monetarist theories and so, to the extent that he discusses the labour market at all, Congdon's story is fairly straightforward:

Congdon Following the argument of Friedman's 1967 presidential address to the American Economic Association and the associated literature, unemployment above the natural rate should cause a progressive amelioration in the unemployment/inflation trade-off as inflation expectations decline. It is this amelioration, combined with the scope for above trend growth because of high unemployment and abundant spare capacity, which justifies optimism about the medium-term outlook[50].

However, it is worth noting that Congdon does not forecast a particularly significant fall in unemployment - in fact his forecast is for virtually no change at all. The important point about Congdon's analysis is that he does not use the idea of hysteresis and consequently believes that inflation will remain low.

Patrick Minford is the only other economist on the Panel who uses this 'Classical' sort of framework. Minford's interpretation of the unemployment prospect is very different from Congdon's in several respects. In particular, Minford sees no reason why unemployment cannot fall substantially (provided, of course, that the right monetary policies are pursued):

Minford If unemployment can fall to 3 per cent or so from its projected 1993 rate of 11 per cent, then output can rise approximately 10 per cent from its depressed 1993 rate: the reasons lie not merely in the greater employment but also in the likely ratio of output to employment which tends to rise in the upswing as productivity increases[51].

Minford likes to think about the labour market in much the same as he thinks about any other market - the price varies so as to equilibrate supply with demand. In the case of the labour market, the price is the wage and the supply is the number of people available for work. At the equilibrium rate of unemployment, the price of labour is such that there is neither excess demand for, nor supply of, labour. Consequently wages remain stable. If there is no hysteresis then the economy will always return to this level of unemployment and, assuming no changes in productivity etc. the same level of output. These equilibrium levels of unemployment and output are the so-called 'natural rates'.

For Minford, this is more or less all there is to the labour market - it is a process by which wages adjust and individuals price themselves into (or out of) work. The goal of economic policy is to create the conditions in which this adjustment can take place as efficiently as possible. What this means in practice is stable economic growth and

social policies to reduce the potency of the factors which adversely affect this adjustment, principally state benefits and union power. Thus, in this respect, Minford's story is not dissimilar to that of Tim Congdon - unemployment can fall and will not be impeded in doing so by inflationary pressures caused by hysteresis effects restricting the supply of labour. The difference between the Minford and Congdon is really in the speed with which these adjustments can occur. As noted in Chapters 1 and 4, Minford favours a new classical, rational expectations approach in which adjustment lags are very short, Congdon favours an adaptive framework within which inertia is typically much greater.

Another difference between Minford and Congdon is the importance they attach to labour market reform and the amount of effort they devote to thinking about it. Here Minford is much more active than Congdon, and has devoted a considerable amount of time to thinking and writing about it.[52] Minford has very strong views on labour market policies and believes that the sort of active labour policies favoured by the mainstream Panellists are mistaken. In particular, he does not accept that the long-term unemployed are excluded from the labour market or that government training programs are necessary to provide the skills needed. Such intervention is unnecessary, he believes, because market forces will, as they have done in the past, ensure that appropriate training programs are provided:

> *Minford* ... whenever we have had skills problems in the past, in the 80s, when there have been skills shortages they have turned out to be very short lived ... I mean, one famous skills shortage was computer software writers. Well they simply raised the wages and people came into software writing just like there was no tomorrow. And now of course there is a glut of computer software writers, which is fair enough, I mean that is the way markets work. So I am extremely sceptical of all this stuff, because it is economics without markets in my opinion. I mean training is very important, [but] it responds to market forces, like anything else does.

Thus, in Minford's world, unemployment will automatically fall to the natural rate fairly quickly but can be held back by government or trade union intervention. In addition, employers and employees can be relied upon to arrange the appropriate training for their needs.

The evidence for hysteresis

The preceding section outlined the most important elements of the labour market theories used by the Panel of Forecasters. The majority use a theory in which the rate of unemployment at which wages and prices will be stable (i.e. the NAIRU) is given by the equation:

$$\text{NAIRU} = \text{Natural Rate} + \text{Hysteresis}$$

Minford and Congdon, on the other hand, believe that the hysteresis component is zero and so their theory can be summarised as:

$$\text{NAIRU} = \text{Natural Rate}$$

Finally, Minford believes that unemployment could fall to the natural rate fairly quickly if the right policies are pursued. Congdon is less clear on the speed at which this adjustment will occur, but as long as unemployment remains above the natural rate, inflationary pressures would remain weak. My aim in this section is to outline some of the ways in which the existence of hysteresis is affirmed or denied and to show how the different theories are made compatible with the same data

The controversy in question here is the existence of hysteresis. Some economists think it exists, others do not. One way of answering the question might be to examine the relationship between unemployment and inflation. Given that the Panel are more or less agreed on what the natural rate of unemployment is (typically 1 - 1.5 million) then, if hysteresis does not exist, inflation pressures should be weak at unemployment rates above this figure. If, on the other hand, hysteresis effects are significant, then inflationary pressures will be strong at unemployment rates around 1-1.5 million and only weaken at higher unemployment rates, say in excess of 2 million. In other words, this would seem to be a testable proposition which offers some way of deciding between Classical and Hysteresis theories.

In fact, the relationship between unemployment and inflation *is* used by those who believe in hysteresis to explain the behaviour of inflation in the UK in the 1980s. For example Andrew Sentance, who acknowledges that the natural rate hypothesis might have been adequate for explaining inflation in the 1960s and 1970s, describes the experience of the 1980s as follows:

> *Sentance* In the 1980s, however, the forces that had contributed to this wage pressure were on the wane. Union membership and influence declined.

> Unemployment benefits also became less generous in relation to incomes in work. These factors should have pushed down the equilibrium rate of unemployment. But their influence was offset by a rising total of labour market "outsiders": long-term unemployment rose steadily, with the number out of work for over a year accounting for almost half the total. As a result, unemployment of over 3 million exerted little downward pressure on inflation, which settled at an underlying rate of 5 per cent in the mid-1980s[53].

David Currie makes a similar point about the late 1980s:

> *Currie* ... what I don't understand is how [Patrick Minford] explains why inflation rose as it did in the 87-88 boom, when unemployment was 1.6 million.
>
> *Evans* Why? Because it was over the natural rate, you think it should have been pushing inflation down?
>
> *Currie* Well, that's the story, that's the way it normally works ... I mean my observation is that it seems to me that the natural rate of unemployment was above 1.6, 1.7 million, probably 2 million plus and I don't really see any good reason why that has changed, indeed if anything it may have deteriorated and may have risen, for hysteresis-type reasons, so the Non-Accelerating Inflation Rate of Unemployment may well be higher.

The case made therefore is that inflation has not been stable at unemployment levels beneath 2 million for the last 10 to 15 years. This then is taken as evidence for one of two things: either, the natural rate of unemployment is about 2 million or, the natural rate is lower, but overlaid on this is additional unemployment due to hysteresis. Clearly, the majority of the Panel favour the latter interpretation, but either way, the argument is that unemployment below 2 million has always been accompanied by rising inflation.

Minford is, of course, aware of these arguments and devotes much of his submission to a detailed rebuttal of them. Put simply, his argument is that the inflation and unemployment figures should not be interpreted out of context. Firstly, he argues that it is not at all clear that wage behaviour was particularly aggressive in the late 1980s, given the headline inflation figures of that time:

> *Minford* According to our story [the acceleration of average earnings growth from 8 to 10 per cent] reflected rising inflationary expectations in the monetary context of 1987-90. It is actually remarkable how little wage settlements

reacted to a sharp rise in inflation (from 5 per cent to 10 per cent on the RPI and around 8 per cent on 'underlying' measures); we explain this by our view that *unemployment was above not below the natural rate.* For all this period real wages were growing by substantially less than the 4.7 per cent 1980s average growth in manufacturing (let alone still higher general industrial) productivity. The behaviour of expected real wages remained moderate even when unemployment had fallen to 1.6 million; this is inconsistent with a natural rate of 2.5 million and above[54].

However, it remains the case that unemployment and prices both rose in the late 1980s and did so at unemployment levels well in excess of the 'natural rate'. If hysteresis is not the reason then what is? It is Minford's contention that the economic policies of the 1980's, particularly the decisions to raise interest rates to keep the UK exchange rate fixed to the German Deutchemark were to blame. In typically flamboyant prose he describes the policies of the 1980s as a violent assault on the UK economy:

Minford ... why did unemployment drop only to 1.6 million and why has it risen to 3 million-plus. Our answer is that owing to our tragic errors in monetary policy we had to hit on the head an economy which otherwise could have remained on a sustained growth path of some 3 per cent. After we had so hit it on the head we joined the ERM proper and continued raining blows on its prostrate body. The resulting deep recession has produced an unemployment 'excess' (over the natural rate) of over 2 million. In short it is recession, not the trends of a poorly-performing labour market, that has delivered us this apparent ratchet[55].

Finally, Minford turns the spotlight back onto hysteresis. One of the most controversial claims of the hysteresis school is that the very fact of being unemployed can make individuals unemployable. According to the mainstream view:

Sentance ... rises in unemployment create a pool of labour market "outsiders" who exert little influence on the process of setting wages and hence on inflation. They also become unattractive to employers as their skills degenerate and as their motivation to work ebbs away. After the deflationary shock has worn off, high unemployment therefore ceases to exert any further downward pressure on inflation[56].

Minford flatly denies that this is the case. Minford's objections to hysteresis are based on two main factors. The first is that there is no convincing (at least to him) econometric proof:

Minford There's no evidence for hysteresis. I mean people have tried to find it and this is what's so pathetic. All the evidence on hysteresis suggests that the most you can find in the unemployment series is a root of 0.7 or something like that, I mean there is adjustment, there is slow adjustment, but there is not a unit root. When unemployment goes up it does not stay up, it slowly declines. The sort of equations that people have been quite clearly of this form

$$u = \alpha u_{t-1} + ...$$

where α is less than 1. It is only if α is equal to one that you get the strong sort of hysteresis. And no-one can find it - it is just not there - there are exhaustive studies by Steve Nickell and Co. on this issue and they have said, basically, that there is slow adjustment. Well I've got slow adjustment in my model, its no big deal. I mean it might include hysteresis reasons, it could well be, although we don't see it like that, its perfectly true. We don't think that the adjustment of real wages, which is what lies behind our adjustment is anything to do with hysteresis. But it could be, it could be. There's lots of reasons for adjustment in the labour market and this could be one ... but the key point is that it has not got a unit root. Its just a fairly long drawn out adjustment, so what's new.[57]

As Minford is prepared to concede, the econometric evidence about hysteresis is not entirely conclusive, and it is possible that a weak form of hysteresis might be one of the reasons why adjustment in the labour market is slow. However, this is not the way that Minford likes to think about it. In fact, Minford has his own theory about why this adjustment is slow, which he explains as follows:

Minford I'm not saying that all the long-term unemployed will get back to work. My whole theory of the labour market, which I published in the middle of the 80s in my *Unemployment: Cause and Cure* book, focuses on the idea that people get caught in the unemployment trap, don't get back to work and stay long-term unemployed because their potential real wage is less than their reservation wage, influenced by benefits. So its no news to me that some long-term unemployed don't go back to work. I'm simply saying that there is no reason to believe that just the fact of being long-term unemployed makes you permanently unemployable. I mean, that position is intellectually indefensible because it has been contradicted by the data. According to that theory the long-term unemployed should have stayed at 1.3 million and all the reduction should have come in the short term

unemployed - those guys should have stayed out of work because they were all disillusioned, deskilled, demoralised blah blah blah - its bollocks, demonstrable bollocks.

Thus, the crucial test for strong hysteresis is to look at what happens to the long-term unemployed. According to the hysteresis argument, being unemployed, particularly long-term unemployed, makes individuals unemployable. According to Minford, this is not what has happened and the long-term unemployed are far more active in the labour market than the mainstream economists acknowledge:

Minford Then we must query the lack of pressure from 'outsiders', in the form of long-term unemployed. Those unemployed more than a year had dropped by end -1990 to 0.5 million from 1.3 million in 1987. Furthermore the turnover rate in the labour market has risen to around 0.3 million per month, approximately 14 per cent of the labour force per year (against 9 per cent in 1988). Hence some 50 per cent of the labour force may have 'quit' jobs and experienced a spell of unemployment in the last four years; even allowing for double and even more frequent spells among these this high rate of activity suggests a wide experience of unemployment in the labour force. This is not a picture of supine labour market behaviour by the unemployed, not even those with the misfortune to become 'long-term' unemployed. Nor would supinity be consistent with the other evidence we have on benefits (now exceedingly low relative to the wages of all but the lowest paid), on the greater vigour with which worktesting (plus job- and re-start programmes) is being applied, and finally the weakness of the traditionally militant unions[58].

What this means to Minford is that:

Minford There is no evidence that once people are out of work they stay out of work for ever. Indeed there is positively contradictory evidence. I mean if you believe in anything remotely Popperian, you cannot sustain the view that long-term unemployed do not get jobs, because it is flatly contradicted by one important event - they did get jobs and they stopped being long-term unemployed. End of story, it seems to me, end of story. I mean you can't even say that they might have not got a job, because they did get a job.[59]

However, despite what Minford says, this is not the end of the story at all. The hysteresis economists focus on those amongst the long-term unemployed who did not get jobs. As Minford would be quick to point out, this is a retreat from the strong form

of hysteresis in which all the long-term unemployed remain out of work. Of course, given that the econometric evidence produced by Nickell and his colleagues suggests only slow adjustment, it is entirely possible that the strong form of hysteresis is a straw man erected for Minford's rhetorical convenience. Either way, by focusing attention on the group which do not get work, the hysteresis economists maintain a space within which special measures to combat long-term unemployment are needed. The reason is that according to the hysteresis argument, the long-term unemployed are simply not competitive in the labour market. If these people could be brought back into the job market, then the supply of employable labour would be increased and its price held down (i.e. inflation would be stable at lower levels of unemployment). Thus, David Currie argues that a scheme introduced as a pilot in the previous Budget, in which the long-term unemployed can use their benefit to subsidise their employment should be widened:

> *Currie* The usual objection to th[is sort of scheme] is 'displacement': you price yourself into a job, but you price somebody else out of a job ... but it seems to me that even if displacement is 100 per cent there is a benefit, because although you have moved somebody into work and somebody else out of work, the person you have moved out of the unemployed into work was, in the long-term, unemployable, and you have given them job experience. So there is some benefit even if there is total displacement, because it is undoubtabley the case, and there is a lot of empirical evidence, that if the unemployed were all short term unemployed, then the sustainable level of unemployment without inflation would be lower.[60]

To summarise what has been said in this section, hysteresis is a controversial topic in labour market economics. The majority of the Panel believe that it exists and that simply being long-term unemployed will render some people (though not all) unemployable. As evidence for this, the hysteresis economists point to the behaviour of unemployment and inflation over the past 10 to 15 years and conclude that unemployment cannot fall much below 2 million without wage pressure leading to a surge in inflation. Consequently this group recommends that measures be introduced to increase the 'turnover' in the labour market and to minimise the number of long-term unemployed. In this way the supply of employable labour will increase, and the stable inflation level of unemployment will fall. Without such measures the future is bleak.

In opposition to this, Minford argues that there is no such thing as hysteresis. The apparent ratchet-like rise in unemployment is, on his account, the product of policy mistakes destroying growth and creating recession. The long-term unemployed are not unemployable, as evidenced by the fact they do get jobs. In Minford's world, the solution is to reduce benefits (and the marginal tax rates faced by those coming off benefits) so that it is actually worth working. In fact, Minford believes that the labour market policies already enacted have made most of the necessary changes, and he therefore believes that, in 1993, the natural rate of unemployment was below 1 million. The future is therefore potentially very promising for Minford, provided that the right polices are followed. The contrast between the two views is nicely summarised by Minford as follows:

> *Minford* They are saying that the labour market will heat up very rapidly the moment the slightest [let-up occurs] and I believe that we are going to see some sort of slow recovery, that's true, unemployment probably falling again, and that we are going to see wages going on plunging ... They are saying that the labour market rigidity and the natural rate are very high, because of insider-power or whatever it might be and so they are taking this view, and I don't believe there's a shred of evidence for it, but that's what they're sticking to. So I am looking forward to that one, because I think the evidence is going to be very strong.[61]

However, this discussion focuses very much on what unemployment is likely to do over the next few years. Also of pressing concern to the Panel was what it had been doing over the preceding few months, in which unemployment had fallen rather unexpectedly. As the remarks by Gavyn Davies made clear, the uncertainty over the current situation made forecasting very difficult. Thus, whatever the theoretical orientations of the economists, it is important that they know where they are forecasting from. It is this discussion which I examine in the next section.

Clutching at straws?

The aim in this section is to show how economic forecasters deal with unexpected events. Throughout this thesis a distinction has been drawn between the general case and the particular instance. It has been argued that econometrics is most useful in those circumstances where the present and the future follow the patterns set down in the past. In Chapter 5, which discussed the forecasts made in February, it was clear that none of the economists forecast a significant fall in unemployment during 1993. Thereafter

their forecasts were for unemployment to stay more or less flat or fall slightly. Only Patrick Minford expected unemployment to fall by more than a quarter of a million by the end of 1994. These forecasts were all based on what had happened to unemployment in previous economic cycles. Typically, unemployment lags the economic cycle, and continues to rise even after economic growth picks up. For example, after the 1981 recession, unemployment continued to rise for a further 2-3 years. In general, therefore, unemployment would not be expected to fall so soon after output stopped contracting. However, this was not what happened in 1993. After rising quite sharply at the end of 1992, unemployment fell in the early months of 1993. As the Panel's report makes clear, the fall in unemployment is a clear deviation from past trends:

> The falls in unemployment in the last four months have come as a surprise to all of us. They have come at a much earlier stage in the cycle than would have been expected in the light of past experience, and the turnaround from the rises at the end of last year was unprecendentedly large. [62]

The problem for the forecasters is thus to adduce specific reasons as to why 1993 is different from similar stages in previous economic cycles. In other words, what needs to be established are the particular features of 1993; the features which, when acknowledged, will both preserve their general model (be it hysteresis or classical) and also account for the discrepancy between that model and the economy. The report contains several hypotheses and these are discussed in more detail below. The least controversial of all the explanations put forward is that the falls represent a correction in the labour market:

> At least part of the reason for the falls in unemployment is that firms took a very gloomy view in the second half of last year [i.e. 1992] and laid off more workers than the subsequent conditions dictated. [63]

Thus, it is suggested that the anomaly is not the fall in unemployment, but the rise which took place at the end of 1992. This rise is then explained as being a collective misjudgement on the part of employers who were unnecessarily pessimistic about the recovery prospect and consequently laid off too many workers. One implication is that if only firms had had a better understanding of the economy, they would have behaved differently. At the very least, if this is the case, then the falls represent a move *towards*, and not *away from*, a more normal state of affairs.

This sort of equilibrium story is no doubt appealing to many economists, but, as the passage quoted above concedes, it is not sufficient to explain all the fall in unemployment. The reason why the 'labour-market correction' explanation is unsatisfactory is that it quite clearly implies that the number of people in work, i.e. the *em*ployed total ought to be rising (because unemployment is falling as people get jobs). However, the increase in employment is not sufficient to match the fall in unemployment; hence the use of the qualifier 'At least part of the reason ...' in the passage quoted above. Additional reasons (i.e. apart from the changing sentiments of employers) are therefore needed to establish the particularities of 1993. The ideas suggested include lay sociology, demographic changes and economics, and they are explained in the Panel's report as follows:

> Other possible explanations [for the falls in unemployment] include:
>
> - the claimant count may overstate the underlying fall, for example if benefit offices are finding it easier to prove that applicants are 'not actively seeking work' now that the economy has turned the corner and there are more jobs around;
> - with unemployment rates more evenly distributed across regions, unemployment may react more quickly to increases in employment opportunities, and thus could fall at a lower level of vacancies than in past episodes;
> - the fall in employment might reflect an increase in firms' desired employment levels due to falling real unit labour costs[64].

Of these three reasons, the first is the most interesting. Advanced by Andrew Britton it is revealing for the amount of lay sociological theory which economists can put into explaining the actions of economic actors.

According to Britton there are two possible interpretations of the unemployment figures. One is that there has been a significant change in the way the labour market responds to the economic cycle; the other is that nothing much has happened at all. Britton seems to favour the latter view, arguing that what we are seeing is a change in the claimant count (i.e. a change in the output of a particular administrative institution) and not a change in economic conditions:

Britton Although we were expecting a rise in the first quarter, we were surprised (like everyone else) by the fall in unemployment. Other labour market indicators do not suggest such a rapid turnaround. Part of the explanation may well be that the relationship between claimant and non-claimant job-seekers changes with the prospects for recovery, as it appeared to do also in the late 1980s. We have therefore revised down our forecast for the claimant count, without substantially changing our view of the real slack in the labour market.[65]

In other words, Andrew Britton is arguing that the fall in the claimant count represents just that - a change in the number of people eligible for a particular state benefit. According to this view, a fall in the claimant count need not reflect the change in the total number of people actually out of work.[66] As a way of justifying this, Andrew Britton refers to the different ways in which the various data for the number of people in or out of work are collected. His interpretation is based on the fact that the ILO Labour Force Survey, which is where the employed total comes from, also includes a figure for unemployment. However, the Labour Force Survey includes as unemployed anybody who has looked for work in the previous month (regardless of their eligibility for unemployment benefit). On this (arguably more inclusive) measure, unemployment has not fallen so dramatically, and is therefore expected to match up with the estimated increase in employment more closely:

Britton ... the first reason given in the main body of the report ... is to the effect that this is a change in the claimant count, which is probably not reflected in what will become the ILO measure, which is the survey based one. Probably you will find that the fall in unemployment on that measure has not been so great.

However, the claimant count has definitely fallen significantly and so there remains a discrepancy which needs to be accounted for - where have the people gone who stopped claiming unemployment benefit, and do not appear in other measures of unemployment but did not enter employment either. One possible explanation is that these 'missing' people have left the labour force altogether and are thus neither 'employed' nor 'unemployed':

Britton We know that there have been a lot of people leaving the measured labour force over a period of a year or two and that these are particularly men approaching retirement age and also people staying on in education or training for longer, so that they are not either employed or unemployed.

> This is the point that was made in the [National Institute] Review, and is backed up by the Labour Force Survey, which runs up to the early months of this year.

However, as Britton admits, it is unlikely that 'the number of claimants, suddenly started dropping [because] a whole lot of people sort of voluntarily left the labour market all in a rush in February, March, April and May'.[67] Rather, he suggests, what may have happened is that a change in administrative procedures within the Department of Employment has resulted in a greater than usual success rate in getting 'getting some of the people they had coached in how to do job interviews and so on, in getting them to the front of the queue'. This could happen without increasing the overall size of the labour force (however measured) because

> *Britton* ... they would have got the jobs that were going, in preference to the people who might have come in from outside the labour force. What normally happens in previous upturns has been that a lot of the jobs at the beginning go to, say, married women looking for part-time work ... The other possibility is that they got a bit tougher on the availability of work test, and it is a bit easier to enforce that test when there are jobs to point people to. As long as there is no work to be had, there is no way of disproving that somebody says they are looking work, [but] if they start refusing offers of employment then they may lose their benefit.

Thus, in order to explain the unemployment, economists resort to a sociological account of how benefit agencies will react under certain social and political pressures. In this way, the peculiarities of 1993 are explained in terms of an administrative change which does not threaten the general model nor have any great implications for the forecasts for other economic variables. In particular, although the claimant count is falling, employment is not rising unusually fast and so neither economic growth nor inflation are likely to pick up much. All that has happened is that the jobs which were going were taken by a different group of people than would have previously been the case.

This is an interesting and quite subtle explanation from an economist who has actually worked for the Department of Social Security.[68] However, it is does not seem to be one which was widely supported by the other Panel members. For example David Currie commented that the 'argument that [Andrew Britton] was trying to advance, the argument that the number of people on the claimants register was falling ... didn't get much support in discussion.'[69]

A similar lack widespread support can be found for the other ideas. For example, the second alternative put forward in the report was that unemployment was less concentrated in any one particular region than had been the case in previous recessions and that this meant that new vacancies were more likely to be filled. Unlike the 'claimant count' explanation, which was based on the premise that nothing much had changed, the demographic explanation is the clear assertion of particular features which differentiate the 1990 recession from previous ones. The distinction being made is that previously unemployment blackspots have been geographically separated from areas of new employment (e.g. the North-South divide). This has meant that new job vacancies have appeared in areas of relatively high employment, and expanded the workforce in certain regions, but done very little to reduce the unemployment in other regions. The claim being made for the 1990s is that this polarisation is much less severe and so unemployed workers and employment opportunities are spatially better matched. However, at least some of the Panel doubt that such generalisation reflect the local picture

> *Britton* ... if you look at actual localities, you still have the blackspots, and people don't seem to travel to work that much. You can get neighbouring areas which have very high and very low unemployment, so that wasn't particularly my idea, it seemed to me quite an original stab at an explanation, but not one that I would particularly want to back.[70]

Although many of the Panel did not talk about forecasts for unemployment in great detail, preferring silence to speculation, it is clear from their forecasts that the falls in unemployment were interpreted as a temporary blip. This is evidenced by the fact that:

> *Britton* ... the group as a whole are not particularly optimistic about unemployment. After having thought of all these explanations, the overall opinion was still that unemployment was unlikely to fall very much more this year. And several of the forecasts actually show it going up again

The implication of this is that the labour force is not responding more quickly, for whatever reasons, to the growth in output than it has done in the past. In other words, the general case remains applicable. The attitudes of David Currie and Gavyn Davies are more or less typical:

> *Currie* I don't feel any strong view on it, so we tended to run with the view that there was an excess shake-out in the autumn and then a correction, a bit, and then we may see some falls and perhaps even a rise. Since we don't really

know what is going on it is very hard to forecast. Its clear that unemployment will be falling by probably autumn, but that is what you would expect anyway.[71]

Davies Considerable uncertainty surrounds the immediate outlook for unemployment. The declines of 26,000 per month in February and March may have been partly due to seasonal adjustment problems, but there have been some confirmatory signals of an improving labour market in other information, including business surveys, and the further fall of 26,000 in May is obviously significant. Although recent figures may have been favourably affected by the fact that too many jobs were shed by many firms last autumn, I assume that the trend has now genuinely turned, and my forecast shows the claimant count falling to 2.9 million at the end of 1993 and to 2.7 million a year later[72].

Finally there was the explanation drawn from economics, according to which the falls in unemployment were the counterpart of a rise in employment. According to this story, falling real wages mean that firms can take on more workers, hence bringing down unemployment. The economist who propounded this view most forcefully was Patrick Minford. His view of flexible labour markets naturally lends itself to this sort of story in which wages adjust quite rapidly to move the economy back towards its equilibrium path.

Minford ... I'm very encouraged actually by what you might call the supply side developments, which seem to be good. The fact that unemployment's fallen ... I think it is people pricing themselves into jobs, in the sense that they are so desperate to get work. They are saying to firms, 'For God's sake, you know, anything you say.'[73]

This 'economics' based account is particularly interesting as it is based on exactly the same evidence as the 'sociological' account, but reaches completely the opposite conclusions. In particular, Patrick Minford was not at all troubled by the discrepancy between the fall in the claimant count and the rise in employment. According to Minford, because the two sets of figures are collected differently they should not be expected to match up exactly:

Minford ... in the first quarter [employment] went up by about 19 000

Evans B[ut not by] the same amount as the unemployment went down?

Minford Not by the same amount no, but then you see, one is a sample and the other is a claimant count, so you wouldn't expect that. But there was a definite rise. One went down and one went up, the exact numbers are not the same. I think the unemployment's gone down by 40 000 or something, and the employment's gone up by 19 000 ... I don't think that that is inconsistent.

In other words, an inconsistency which demands explanation in the National Institute is not an inconsistency at all in Liverpool. Moreover, the implications of these two views are quite different. In the more sociological economics of the National Institute, the patterns of the past are expected to be repeated, and so unemployment falls only slowly. In the flexible economy of the Liverpool model, the falls in unemployment are caused by growth in the economy and reflect the increased flexibility of the UK labour force - there is therefore every reason to expect that the falls will continue. But, Andrew Britton points out:

Britton If that is the interpretation, that the people who are unemployed now are more flexible, then the outlook for getting unemployment down over the next year or so would also be better. It is not just an explanation of the first few months, but it would be a reason for optimism [more generally]. But the group as a whole are not particularly optimistic about unemployment.[74]

Minford, as we have seen, however is confident that, with falling real wages, output can grow and unemployment fall without inflationary pressures:

Minford There is definitely a rise in employment in the first quarter, so I think these people are just going to be proved to be much too pessimistic about the operation of the labour market[75]

Conclusions

The preceding section have outlined some of the main features of the economic debates over employment and unemployment. The discussion has included both theoretical and practical issues highlighted by economists. From a theoretical perspective, the controversy over the importance of hysteresis was discussed and it was shown how neither side can convince the others. For example, the allegedly powerful relationship between unemployment and inflation was deconstructed by Minford and shown to be the product of poor policy making. Similarly, econometric evidence was also shown to be less than convincing.

On a more practical front, some of the problems associated with forecasting unemployment were discussed. It was shown how the general propositions about unemployment were maintained in the light of contradictory experiences and how novel strategies, drawing on a range of non-economic evidence were developed in order to account for these. In addition, the same data was also consistent with two different explanations, and some of the implications of the differing views were highlighted.

The PSBR, Trade Deficit and Fiscal Policy in the Medium Term

The other important topic discussed at the July meeting was the medium term outlook for the UK economy. This was a topic over which there was considerable disagreement and which exposed the fragile nature of the consensus reached in the February meeting, (at which 6 out of the 7 had agreed that fiscal policy should not be tightened during 1993). Although the debate was ostensibly about what would happen to the Public Sector Borrowing Requirement (PSBR) over the next few years, it also touched on other important issues, including the rationale for macroeconomic policy itself. The debate is interesting because it highlights very clearly how different policy and ideological priorities can colour interpretations of the same data. This section begins with a brief summary of the PSBR position in mid-1993 and highlights some important links between the PSBR and unemployment and net trade, both of which influence its future path.

As discussed in Chapter 5, public spending in the UK had increased relative to the revenue from tax receipts during the recession. As a result, the government had been forced to increase its level of borrowing in order to meet its expenditures. The PSBR for the year 1993-94 was forecast by the Panel to be between £41 bn and £52 bn. However, this, in itself, was not really the problem. As Andrew Britton remarked in his submission to the Report 'a large borrowing requirement is the correct response to a long and deep recession.'[76] In addition, with the recession now over, the expectation would be that the PSBR would fall as the economy picks up. This process is very much part of the general pattern of economic events and, as such, was anticipated by all the forecasters:

> Public finances lag the cycle. Thus, despite the evidence that the recovery is underway and that employment prospects may be improving, we all expect a worsening of the **PSBR** in 1993-93. However, the average of our forecasts is below the Government's projection of £50 billion. Cyclical

improvement, coupled with the tax changes already announced, mean that we all forecast a reduction in the PSBR in 1994-95, but only Minford expects a dramatic change. His forecast is conditioned by an assumed reduction in public spending in 1993-94 followed by severe restraint[77].

The reduction in the PSBR should come from several sources, the most important of which were identified by Andrew Britton as follows:

Britton At this stage in the cycle, the finances of the public sector should improve quite sharply, because company sector taxation, which is the most cyclical part, actually lags behind because they pay so much in arrears and, in a year or so's time, there ought to be a substantial increase in corporation tax coming from the improved profits which are earned this year. And that is a very important component of the cyclical part of the borrowing. And similarly with unemployment. Normally unemployment wouldn't be falling until the recovery is well underway, but as soon as unemployment starts falling that is a significant saving on expenditure. It is actually not only unemployment benefit but when the labour market picks up, you know all this discussion about invalidity benefit and one parent benefit, there are jobs for people and they come off a number of benefits, as well as unemployment benefit. Or people who are essentially out of work get put on invalidity benefit rather than unemployment benefit, because they are slightly ill, but when there is a boom in jobs, their disability will not actually prevent them from working. So it is really a matter of social security generally being very highly geared to the state of the cycle. So if we have got any perceptible recovery at all, we ought to get the public sector finances improving in the next couple of years.

A similar point by Patrick Minford was highlighted in Chapter 5 in which the PSBR was also discussed and Andrew Sentance has also suggested that for every 1 per cent growth in GDP, the PSBR should be reduced by half a per cent of GDP.[78] In other words, this proposition is not at all controversial.

At the February meeting, the discussion of the PSBR was concerned mostly with policy implications of the large PSBR. At that time, the debate centred on the issue of how much of the deficit was cyclical and how much was structural. The consensus then was that there were elements of both and that taxes would have to rise in the future, although only Currie and Davies wanted the measures to be spelt out in advance.

In July, after the Chancellors March Budget in which the sort of post-dated tax increases favoured by some of the Panel had in fact been implemented, the Panel returned to the topic of the PSBR once again. In the July meeting the focus was slightly different from February. Although there was some discussion as to whether or not the tax increases announced were sufficient to ensure an appropriate deficit in 3 to 4 years time (an almost impossible question to answer) the Panel also wanted to connect the PSBR to other problems in the economy, in particular to the so-called 'twin-deficit' problem. By doing this, questions of fiscal policy, which had previously been related solely to the need to reduce PSBR, become enmeshed in other debates about the future development of the UK economy.

Has Enough Been Done?

In their February Report, 6 out of the 7 Panel members agreed that fiscal policy ought not to be tightened in 1993, but that some policy response would probably be required later. It should be noted that Patrick Minford actually thought that the PSBR was cyclical and would therefore disappear without any special measures. Nevertheless he actually assumes that there will be a fall in real public expenditure 1993-94 followed by severe restraint in subsequent years, and so he does, implicitly, forecast a tightening of fiscal policy. The only dissenter over fiscal policy in the February Report was Tim Congdon, who agreed that tax increases were necessary, but wanted the process to start straight away.

In making recommendations about the future direction of policy, one important consideration is the assessment of what has already been announced. In the March Budget, the Chancellor announced what were generally regarded as fairly tough spending limits, together with a series of tax raising measures, including the widening of the VAT base to include fuels. In line with the Panel's recommendation, none of the fiscal measures were to be implemented in 1993. How then did the Panel react to the Budget?

In general, the Panel did not think that the measures proposed by the Chancellor had done enough to ensure an adequate reduction in the public deficit and four out of the seven called for additional fiscal tightening

> Congdon, Currie, Davies and Sentance all advocate further discretionary tightening in policy to reduce the deficit by 1-1.5 per cent of GDP. This would provide greater certainty that the debt position will become

sustainable. For some of this group, the measures should be phased in gradually[79].

Of the remaining three, Britton is also somewhat sympathetic towards this view. His view, which is consistent with his reluctance to pre-announce policies in February, is that there may be reasons why the government would prefer to take such decisions in 1993 (e.g. rather than in the run-up to an election), but that such policies are not really necessary yet.

Britton the natural buoyancy of the revenue in the recovery, even if it is not a very long recovery, should get us back to the position where we are still borrowing, but not borrowing at an explosive rate. So I'm not pressing for further tax increases ... I am quite prepared to accept that the government may prefer to put up taxes. I don't feel that they have to, but they may prefer to up taxes, in which case it is very important that they should accompany that with a cut in interest rates in order to maintain the strength of the recovery.

Similarly, those members of the Panel who do advocate further fiscal tightening recognise that it is not clear that the policies can be justified simply in terms of the Budget deficit. For example, Gavyn Davies writes:

Davies On my central projections for GDP growth, the PSBR would 'automatically' fall to about 4 per cent of GDP by 1997/98 - not far from a reasonable medium term target, which I suggest should be around 3 per cent of GDP (with a debt/GDP ratio of 60 per cent). This suggests that the need for further fiscal action to reduce the PSBR may be rather small, and is certainly swamped by the huge uncertainty in the forecast ... I argued in my last Panel report that a risk-averse Chancellor would introduce now measures to limit the PSBR in later years ... If these measures eventually proved unnecessary to control the PSBR, they could be offset by income tax cuts later, in which case a desirable economic reform would have taken place.[80]

Thus, attempts are made to minimise the importance of the uncertainty which surrounds forecasting the PSBR 4 years in advance and to articulate other reasons why fiscal measures which could help to ensure sustainablity of the public finances will not prove to be detrimental for other reasons.

Of the other two economists, Minford remained adamantly opposed to tax increases in the future, arguing that the deficit was cyclical. By extending the horizon of the discussions beyond the Budget, the July report exposed the rather contrived nature of the consensus produced at the previous meeting:

> *Minford* [Last time we were fairly unanimous] about not having taxes this year, but having them next year, although I didn't want them next year. We at least agreed we shouldn't have them this year which is what seemed relevant at the time, with the Budget coming up. But this time [i.e. July 1993] we were in next year, as it were, and so obviously could not patch over that one any more, and it came right out of the woodwork. Some people were in favour of big tax increases; others saying no, what's done is enough; others saying no, you shouldn't increase taxes at all, you should cut spending over a reasonable period of time.[81]

Finally, Wynne Godley also opposed tax increases, but for very different reasons. Godley's position, as outlined in Chapter 4, was that the UK economy was constrained by its inability to successfully export. In addition, domestic demand was very weak because consumers were saving and paying off debts. Given this, Godley, argued that reducing the disposable income of consumers through increases in taxation would only further weaken demand, thereby threatening an already precarious recovery. Godley's view was that measures to improve export performance were vital, so that the economy could grow sustainably, and that fiscal policy would have to be tightened at the point where consumers' increased expenditure began to increase imports. He warns, rather bleakly, that this process could lead to very large increases in taxation:

> Godley thinks the problem is more intractable than suggested by the other Panel members. The PSBR has become largely structural because Britain's foreign trade performance is unlikely to warrant growth fast enough to have much impact on the public finances. However, taxes should not be put up while the present unusual excess of private income over expenditure persists. When the private sector surplus does fall, fiscal policy will have to be tightened if a larger balance of payments deficit is threatened. The eventual scale of this tightening might have to be very large indeed[82].

This link between the trade deficit and the budget deficit was also made by several of the other forecasters and is referred to as the 'twin deficit' problem. As might be expected, the Panel differed over the importance of the two deficits, but several were concerned. The ways in which the trade deficit was discussed is analysed below.

The Twin Deficits

This third issue was probably the most contentious of all the public finance related debates. As shown above, if tax increases were simply for the PSBR, then they could also be offset if the deficit disappeared more quickly than expected. The problem identified by Godley, and backed by several of the others, was much more serious in terms of the time and resources it would take to put right, but also in terms of how easy it would be to undo.

The proponents of twin deficit problem essentially argued that, if past patterns were repeated, then increases in GDP sufficient to bring down unemployment much below 2 million would either push up inflation (thus requiring deflationary polices) or increase imports, and hence the trade deficit, to such an extent that interest rates have to rise to prevent a 'sterling crisis'. On the other hand, if GDP growth (and thus imports) were to be restricted such that the trade deficit did not become a problem, then growth would be so slow that unemployment would not fall at all. As a result of this continued high unemployment the PSBR would continue to grow and thus further tax increases would be needed.

The issue of the twin-deficit problem was important in the July meeting because it is closely connected with the forecasts for unemployment and for the growth in output. In the discussion of unemployment and the importance (or not) of hysteresis, it was shown that the majority of the Panel thought that growth in output would be limited because of the inflation which would occur as unemployment dropped below 2 million. It is a corollary of this that if unemployment remains high, then so must the PSBR. On the other hand, for those who believed that unemployment could fall to around 1 million without inflation picking up, then the deficit problem did not exist. As Andrew Britton remarked:

> *Britton* The question about how far [the public sector finances] will improve is of course entirely bound up with how far you think the recovery can go ... the optimists on the natural rate of unemployment are also optimists on public borrowing; the pessimists on sustainable growth are pessimists on public borrowing.

Thus, Patrick Minford is the least impressed with this argument. He believes that its basic premise, that labour market rigidities are such that unemployment cannot fall significantly without inflation rising, is simply incorrect:

Minford I mean the sorts of factors that David Currie and Andrew Britton are talking about, the labour market being difficult, I believe that in the next six months we are going to see that that's completely wrong.[83]

Similarly, on the question of net trade, he sees no reason why the UK can not compete effectively in world markets. Although he forecasts a stable rather than a decreasing current account deficit, this is in the context of sustained and non-inflationary economic growth. The difference between Minford's views and the rest of the Panel is difficult to analyse in terms of exports-vs.-imports as he does not forecast them separately, but only as a joint function. Nevertheless, the important point is that he does not believe, as do the majority of the Panel, that above trend growth will result in increased imports. Minford summarises his position as follows:

Minford ... our forecast for the current account is running at levels not dissimilar from the present levels against a background of quite sustained growth, you know 3.5 per cent, for a long long time to come. This obviously reflects import penetration as the economy recovers. And the reason the economy is growing, is that it is driven by potential output and the natural rate [of unemployment] being lower than the actual rate. So people are getting back to work and producing.[84]

The other optimist on the balance of payments is Tim Congdon. Like Minford his story is based on the belief that the UK is a long way below potential output and above the natural rate of unemployment. The growth/inflation combination figures he forecasts reflect this. However, Congdon's forecasts for the balance of trade are also low, principally due to a fall in imports which the more pessimistic members of the Panel do not foresee. For example, Wynne Godley notes that for Minford and Congdon's forecasts to be consistent they 'are (must be) assuming a growth in net export demand far in excess of anything that has ever occurred in the last fifteen years'.[85]

In addition, Congdon is also on record (see Chapter 5) as saying that the Balance of Trade Deficit ought not to be a matter for government concern, so long as its own finances are in order. It is Congdon's belief that the trade deficit represents the actions of free economic agents and should not therefore be subject to political interference. However, it is exactly this point with which the pessimists disagree. Not only do they forecast a widening trade deficit, asserting that the general patterns of the past will be repeated, they also believe, *pace* Congdon, that this is a problem. Godley and Davies, the two strongest proponents of this view articulate their concerns as follows.[86]

Godley Whether or not a balance of payments deficit 'matters' seems, unfortunately, to still be a vexed question. To my eye it is obvious that it does matter if only because no country can get into debt to an unlimited degree.[87]

Davies 'Automatic' financing [of the trade deficit] will only occur for as long as foreigners are content to accumulate UK assets ... However, as soon as growth falters, or inflation threatens to rise, the existence of a large trade gap becomes quite menacing for the economy. Suddenly the flow of foreigners willing to acquire UK assets, and lend to British citizens can dry up. Sterling can then fall sharply, forcing up interest rates. Or the sudden unavailability of foreign lending can lead to a severe dislocation as domestic spending has to be curtailed. Either way, the correction of the trade deficit involves a recession.[88]

However, Godley is also quick to point out that the current balance of trade figures are also important for other reasons. The argument he advances is that, if the PSBR is small (which it should be) and consumer expenditure conforms to normal patterns, then the balance of trade is given by the following identity:

Balance of Payments = Budget Deficit - Private Sector Surplus

In other words a large balance of payments deficit implies either a large budget deficit or an excess of borrowing by the private sector. Since neither of these option is sustainable, it follows that the Balance of Payment is important. In fact, in Godley's economics, the balance of payments is more than important, it is absolutely crucial and is given a key causal role. His policy advice is that 'the government should attach extremely high priority to achieving a rapid expansion in net export demand.'[89] This follows from the identity given above: if net trade improves, then the economy can grow and there will be no need for any fiscal tightening - if the Balance of Payments deficit is small, then the Budget Deficit is also small[90]. However, the weak state of the European economies means that the export market is very weak and so any growth in exports is also likely to be weak. In such a situation the PSBR remains high and can only be brought down with a substantial increase in taxation.

Like Godley, the mainstream economists on the Panel believe that a poor export performance and a high propensity to import is a serious weakness in the UK economy. Similarly they agree that it is something which governments ought to (and could do) something about. However, they differ in that they do not give the same priority to net

trade performance as Godley. The concept the mainstream economists use to frame the issue is that of the 'balance of the economy'. The 'balance of the economy' is based upon a slicing-the-cake metaphor: if one person has too much then there is not enough left for everyone else. In the case of the UK economy, the 'greedy-guts' is consumption and the losers are net exports and investment. The policy solution therefore, which is quite independent from the sustainablity of the PSBR, is that reducing consumption as a proportion of GDP increases the portion of the national cake available for exports and investment.

In fact, five out of the seven Panel members thought that this imbalance between consumption and investment was important and they suggested a variety ways of addressing it. They all focused on three points:

- policies are needed to retrain the long-term unemployed, reduce long-term unemployment (and hence hysteresis) and restrain wages;
- training and skill levels in the rest of the economy need to be improved to increase productivity in the economy as a whole;
- the spending and taxation priorities need to be changed so that the first two points can be achieved.

As might be expected the specific policies advocated varied, both in the amount of extra taxation required and in the mechanism through which the policies would bring about the goals realised. However, the one thing which all shared was that these were long-term policies which required a change in the political culture if they were to succeed.

The two economists who have been most involved with these policy issues are probably Andrew Britton of the National Institute and Andrew Sentance of the CBI[91]. Interestingly both argued that increases in taxation were not necessary to bring about the desired changes:

> *Britton* At this stage I would not argue for tax increases beyond those already announced, or spending cuts beyond those necessary to keep within the totals already agreed. What is required however is a change in the structure of both spending and taxation to encourage growth of output and especially of employment. Other objectives might have to be sacrificed if that aim is given priority.[92]

Sentance I think that if we want more [Research and Development or investment], we can't just say that the overall scale of government activity has to increase further in order to do it. I think there has to be some re-orientation of priorities within existing government programs. Now I think many of the, some of the members of the group take it as a sort of given that it is very difficult to restrain government spending, and you can't do that, but I just don't take that view.[93]

Despite the similarity if their views, Britton and Sentance do differ on the mechanism involved. Andrew Sentance sums up the differences as follows:

Sentance [Andrew Britton] obviously believes in more government activity in the economy in general, because he obviously believes that the money flows into taxes and then via the government into investment ... I would say that if the government tightens its fiscal tax, i.e. narrows the gap between government spending or government consumption and taxation, then that will allow it to reduce interest rates, which should enable private investment. He seems to think that it all takes place within the government sector.[94]

Gavyn Davies and David Currie tend to follow Sentance and Godley by arguing that the aim should be to keep interest rates low and restrain demand with fiscal policy. However, unlike Sentance, David Currie and Gavyn Davies, take the view that additional taxation would be necessary. Currie is the more moderate of the two, justifying a fiscal tightening of 1.5 per cent of GDP as follows:

Currie ... total consumption, counting both private and public is at too high a level ... With so much of GDP devoted to consumption, there is insufficient room for investment and net exports. For last September's devaluation to be made to work, in terms of channelling more resources to the balance of payments to reduce the external deficit, total consumption needs to be held back to make room for net exports. This may be done by cutting public consumption or curtailing private consumption by tax increases ... Public spending cuts are a natural choice, but the danger is that they can be indiscriminate, cutting into areas of spending that are important for promoting longer term growth, for example, spending on education and training, R&D, and infrastructure. Indeed, it can be argued that in these areas there is a need for increased spending to promote the international competitiveness of UK industry ... Because of this, the Chancellor may have to consider increases in direct taxes, along with other measures.[95]

Davies on the other hand foresees the need for a fiscal tightening of 'at least 2-3 per cent of GDP', one half going to reduce the PSBR and the other to fund special employment training and other infrastructure programs.

> *Davies* It is now extremely well known that the trade deficit is unusually large for the present stage of the economic cycle ... The share of personal consumption in GDP is correspondingly large - around 66 per cent, compared with a long-term average prior to 1985 of about 60 per cent. There is a similar "excess" of real personal disposable income in GDP at the present time ... Ideally, we need to shift resources out of short term consumption (public as well as private), and into both exports and investment ... Since the government has recently re-affirmed its medium term spending targets at unchanged levels, higher taxation would need to bear the brunt of this action.[96]

Thus, we can see that five of the seven Panel members felt that net exports were weak and that some remedial action was necessary. In addition, four out of the seven felt that not enough had been done to secure the sustainabilty of the PSBR. Godley was agnostic on this latter point as it is, in his analysis, a subset of the trade problem. Nevertheless, it was his 'opinion that ... the growth of net export demand over the next few years will not, on present policies, be any faster in the future than in the past and hence it will not be possible to achieve sustainable growth fast enough to reduce unemployment by any significant amount over the next four to five years.'[97]

There was therefore a fairly clear majority for policies to improve competitiveness and hence net exports (increased training etc. for most, increased demand for Godley). In addition, there was a weaker majority for a further tightening of fiscal policy, with Andrew Britton agreeing that it would be understandable if taxes were to be increased.

Conclusion

Three separate strands of argument have been identified in the debate about the public finances. The first is that there is not a problem at all and that low unemployment, a balanced budget and acceptable trade figures are to be expected. The second strand is that the fiscal tightening proposed in the Budget was not sufficient to ensure the sustainability of the PSBR and that additional measures were needed. The third strand of argument was that, even if the PSBR situation had been adequately dealt with, there

were other reasons why further tax increases were desirable. These sets of arguments about fiscal policy, together with their proponents, are summarised in the Table below:

Table 1: Views on Fiscal Policy

No Problem	Structural Deficit Remains	Balance of Demand is Wrong
Minford	Congdon	Godley
	Currie	Davies
	Davies	Britton
	Sentance	Currie
		Sentance

The table is particularly interesting if it is compared with the policies eventually adopted by the government. It is fair to say that none of the concerns raised by the Panel were high on the public agenda at the time - the question is therefore did they become more important as a result of the Report. The answer to this seems to be yes and no. In some respects policy later moved in the opposite direction with some of the tax increases proposed in the March Budget being voted off the statute books before they even became law (e.g. the increase of VAT on fuels from 8 to 17.5 per cent). On the other hand, as will be discussed in the next chapter, further action *was* taken to ensure that the PSBR was reduced. A detailed analysis would require following the developments in net trade, GDP, unemployment and the PSBR for several years to see if these have in fact developed in such a way as to prove the pessimists wrong and to negate the need for the policies they recommended. Although it has not been possible to do this in any detail, the figures for the UK can be derived from the forecasts contained in the Panel's Reports, and these are displayed in Table 2, overleaf.

From the Table it can be seen that the Balance of Payments, the PSBR and unemployment have all come down faster than the majority of the Panel expected and that the twin-deficit problem was (perhaps) not really a problem after all[98]. In the case of the PSBR this is a result of poliey, but the same cannot be said of the Balance of Trade deficit. However, there was no way of knowing this in advance and the policy non-decisions of the government can not be justified by a whiggish appeal to what later turned out to be the case. So why did the government not act on the recommendations of advisors which it itself had appointed and attempt to shift the balance of demand?

Table 2: July 1993 Forecasts and Outturns for key economic variables

Forecaster	Forecasts in July 93 for:	GDP Growth (%)	Unemploy-ment (m)	PSBR (% of GDP)	Current Ac. (% of GDP)
Britton	1993(-94)	2.0	2.96	7.2	-3.3
	1994(-95)	2.8	2.89	6.2	-2.9
	1995(-96)	2.7	2.78	4.9	-2.7
Currie	1993(-94)	1.5	3.03	8.2	-3.0
	1994(-95)	2.8	3.03	6.3	-3.0
	1995(-96)	2.7	2.91	5.0	-2.8
Davies	1993(-94)	1.7	2.86	7.6	-3.0
	1994(-95)	3.2	2.71	5.8	-2.6
	1995(-96)	3.0	2.47	4.7	-2.2
Godley	1993(-94)	1.2	3.00	6.8	-2.9
	1994(-95)	1.3	3.00	6.4	-2.7
	1995(-96)	1.5	3.10	5.2	-2.1
Minford	1993(-94)	1.5	2.90	6.7	-1.8
	1994(-95)	2.8	2.50	3.3	-1.9
	1995(-96)	3.1	2.20	1.7	-2.1
Sentance	1993(-94)	1.6	2.90	7.6	-3.0
	1994(-95)	2.6	2.90	6.3	-2.6
	1995(-96)	3.0	2.80	5.3	-2.8
Outcomes	***1993(-94)***	***1.9***	***2.9***	***7.1***	***-1.7***
	1994(-95)	***3.9***	***2.6***	***5.3***	***-0.3***
	1995(-96)[1]	***2.7***	***2.3***	***3.8***	***-0.9***
Forecasts	***1998(-99)***	***2.8***	***1.8***	***1.6***	***-0.7***

[1] Outturns derived from forecasts produced in November 1995

One explanation is that the long-term view taken by the Panel was simply incompatible with the short-term outlook so vital in 'soundbite' politics. For example, with an election due in the next few years, such a major change of policy was always going to be difficult to sell. In addition, the implications for future tax rates (at least on the analyses of Currie and Davies) would have been ideologically unacceptable to a Conservative government, especially one with a small majority. However, the trouble with all these sorts of explanations is that they tend to put the 'blame' on the government - it is the needs of the political cycle, their ideological baggage and so on which have prevented the scientific advice of its experts from being followed.

But what if the advice offered to the government was not judged to be sound enough for such momentous decisions? An alternative and surely plausible account is that the government made a 'rational' decision to delay acting upon the advice until it was clearer that such major, and potentially unpopular policies, would be accepted. This process is not well described as political machiavellianism - it was on the advice of economists, amongst others, that the UK joined the Exchange Rate Mechanism. It is therefore entirely plausible that the credibility of the forecasters was too low for their

advice to taken seriously. In other words, the expertise of the experts was not, anymore, sound enough for them to be trusted. However, if this is the case, what purpose does the Panel serve? From this perspective, the role of the Panel is not to restore credibility to an incredulous government, but to give economists to chance to regain the trust of the policy makers.[99]

Notes

[1] *The Panel of Independent Forecasters, July 1993,* H.M. Treasury, para 1.

[2] op cit. note 1, para 3.

[3] op cit. note 1, para 7.

[4] Andrew Britton, *Submission to Report of Panel of Independent Forecasters, July 1993*, para 4.

[5] Tim Congdon, *Submission to Report of Panel of Independent Forecasters, July 1993*, para 10.

[6] David Currie, Submission to *Report of Panel of Independent Forecasters, July 1993*, para 1.

[7] op cit. note 1, para 13.

[8] David Currie appears to be a slight outlier with a forecast of 2.4 per cent. However, the Panel themselves do not see this as being significantly different from the other forecasts, as is made clear in the passage cited. The reason is probably that, given the uncertainty which surrounds the future path for the savings ratio, the uncertainty which surrounds any forecasts comfortably includes all the rest, including Currie's.

[9] In February, the forecasts ranged from 0 per cent to 1.6 per cent, in July six of the seven forecasts lay in the range 1.2 to 1.8 per cent. Currie, as noted above (note 8) forecast an increase of 2.4 per cent.

[10] op cit. note 1, para 9.

[11] op cit. note 1, para 10.

[12] Andrew Britton; interview; 12 July 1993; pp 10-11.

[13] David Currie; interview; 13 July 1993; p. 4.

[14] Andrew Sentance; interview; 13 July 1993; p. 13.

[15] Presumably they had not read the National Institutes forecasts!

[16] Andrew Sentance; op cit. note 14; p. 14.

[17] David Currie, op cit. note 6, para 16.

[18] Gavyn Davies, Submission to *Report of Panel of Independent Forecasters, February 1993*, para 7.

[19] Gavyn Davies, Submission to *Report of Panel of Independent Forecasters, July 1993*, para 1.

[20] Tim Congdon, op cit. note 5, para 10.

[21] Gavyn Davies, op cit. note 19, para 12.

[22] Gavyn Davies; interview; 27 October 1993; p. 5.

[23] Gavyn Davies, op cit. note 19, para 15.

[24] Gavyn Davies, op cit. note 19, para 15; emphasis added.

[25] Gavyn Davies, op cit. note 19, para 1.

[26] Gavyn Davies, op cit. note 19, para 47.

[27] op cit. note 1, para 18.

[28] David Currie, op cit. note 6, para 10.

[29] Andrew Sentance, *Submission to Report of Panel of Independent Forecasters, July 1993*, para 7.

[30] Gavyn Davies, op cit. note 19, para 21.

[31] Gavyn Davies, op cit. note 19, para 21.

[32] Andrew Britton, op cit. note 4, para 6.

[33] Andrew Britton, op cit. note 4, para 7.

[34] David Currie, op cit. note 6, para 17.

[35] Tim Congdon, op cit. note 5, para 6.

[36] Patrick Minford, Submission to *Report of Panel of Independent Forecasts, July 1993*, para 23.

[37] Patrick Minford, op cit. note 36, para 24.

[38] Patrick Minford, op cit. note 36, para 28

[39] Patrick Minford, op cit. note 36, para 29.

[40] Patrick Minford, op cit. note 36, para 31.

[41] Gavyn Davies, op cit. note 19, para 20.

[42] See e.g. Layard, R., Nickell, S. and Jackman, R. (1991) *Unemployment: Macroeconomic Performance and the Labour Market.* Oxford and New York: Oxford University Press; Layard, R., Nickell, S. and Jackman, R. (1994) *The Unemployment Crisis.* Oxford: Oxford University Press

[43] Andrew Britton; op cit. note 12, pp. 3-4.

[44] Andrew Sentance; interview, 13 July 1993, p.15.

[45] Although they can be shifted off unemployment benefit and onto income support or other benefits. When this happens they are no longer included in the Claimant Count definition of unemployment, but nevertheless remain out of work.

[46] Andrew Britton; op cit. note 12, p. 7.

[47] David Currie; op cit. note 13, pp. 6-7.

[48] Andrew Sentance, op cit. note 14, pp. 15-16.

[49] See e.g. Tim Congdon, Lombard Street Research Ltd *Quarterly UK Economic Forecast*, February 1993, p. 20.

[50] Tim Congdon, op cit. note 5, para 2.

[51] Patrick Minford, op cit. note 36, para 13

[52] See: Patrick Minford (1985) *Unemployment: Cause and Cure, 2nd ed.* Oxford, UK ; New York, NY, USA : Basil Blackwell.

[53] Andrew Sentance, op cit. note 29, para 17.

[54] Patrick Minford, op cit. note 36, para 17.

[55] Patrick Minford, op cit. note 36, para 19, emphasis added.

[56] Andrew Sentance, op cit. note 29, para 15.

[57] Patrick Minford; interview, July 1993, p. 13.

[58] Patrick Minford, op cit. note 36, para 18.

[59] Patrick Minford; op cit. note 57, pp. 13-14.

[60] David Currie; op cit. note 13, pp. 6-7.

[61] Patrick Minford; op cit. note 57, pp. 6-7.

[62] op cit. note 1, para 11.

[63] op cit. note 1, para 11.

[64] op cit. note 1, para 11.

[65] Andrew Britton, op cit. note 4, para 5.

[66] Note that the National Institute does not consider unemployment to be the most useful measure of excess supply in the labour market. See Chapter 1.

[67] And he should know because he worked for the DSS

[68] Andrew Britton, op cit. note 12, p. 18.

[69] David Currie, op cit. note 13, p. 5.

[70] Andrew Britton, op cit. note 12, p. 12.

[71] David Currie, op cit. note 12, 5.

[72] Gavyn Davies, op cit. note 25, para 20.

[73] Patrick Minford, op cit. note 57, p. 7.

[74] Andrew Britton, op cit. note 12, p. 13.

[75] Patrick Minford, op cite note 57, pp. 7-8.

[76] Andrew Britton, op cit. note 4, para 13.

[77] op cit. note 1, para 19.

[78] Andrew Sentance, op cit. note 14, pp. 9-10.

[79] op cit. note 1, para 36.

[80] Gavyn Davies op cit. note 19, paras 4 and 44

[81] Patrick Minford, op cit. note 57, p. 5.

[82] op cit. note 1, para 37.

[83] Patrick Minford, op cit. note 57, p. 5.

[84] Patrick Minford, op cit. note 57, p. 15

[85] Wynne Godley, *Submission to Report of the Panel of Independent Forecasters, July 1993*, para 7, fn 3

[86] This is a point disputed by Congdon, who has argued that the deficit, as a proportion of national assets is tiny. See Congdon, T. (1993) 'A Critique of International Micawberism', *Economic Affairs*, November/December 1993, pp 13-18.

[87] Wynne Godley, op cit. note 85, para 7

[88] Gavyn Davies, op cit. note 19, para 30-1

[89] Wynne Godley, op cit. note 85, para 17.

[90] This assumes that the Private Sector Surplus is also small, which it usually is. However, it was both large and negative in 1993.

[91] See: Andrew Britton (1993) 'Two Routes to Full Employment' *National Institute Economic Review* (May 1993) pp. 5-11. Andrew Sentance has also written about the 'investment gap'. See: Sentance, A. (1992) 'Rebalancing the British Economy' *The Business Economist*, Vol. 24, No. 1 (Winter 1992), pp. 22-32.

[92] Andrew Britton, op cit. note 4, para 25.

[93] Andrew Sentance, op cit. note 14, p. 12.

[94] Andrew Sentance, op cit. note 14, p. 11.

[95] David Currie, op cit. note 6, paras 18-20.

[96] Gavyn Davies, op cit. note 19, paras 51-3

[97] Wynne Godley, op cit. note 85, para 16

[98] Of course, these forecasts could be wrong too!

[99] A similar point is made by Theodore Porter who argues that the increasing tendency, especially in the US, for decisions to be made according to quantitative decision criteria (e.g. cost-benefit analysis) reflects not increasing objectivity but increasing distrust which then results in strategies to limit the discretion of advisers. See Theodore Porter (1995) *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* Princeton, N.J.: Princeton University Press.

Chapter 7

This chapter outlines some of the issues raised when the Panel of Forecasters met for the third time, in October 1993. The meeting was important for several reasons. Firstly, it was the last chance for the Panel members to revise their forecasts for the rapidly approaching 'day of judgement' at the end of the year. With only about 12 weeks to go before 1993 became 1994, it was clear that some of the more bleak forecasts made in February were in need of revision. Less kindly, it could be said that they were wrong.

The October meeting was also important because it preceded the UK's first ever 'Unified' Budget, in which the Chancellor would be announcing both his tax and spending plans simultaneously.[1] In other words, the future direction of economic policy was once again the topic of debate and, for the second time since the exit from the ERM, the Panel were being called upon to give policy advice to the Chancellor.

There was also a third way in which the October meeting was unusual. In September 1993, the Central Statistic Office had issued a revised set of economic data for the UK. The new dataset incorporated several changes: all data was now expressed in terms of 1990 prices, rather than 1985 prices; the trade figures were now calculated on the basis of VAT receipts, rather than figures provided by H.M. Customs and Excise; and the profile of several data series, including GDP, had been altered. These changes caused some problems for the Panel. Not only had economic history literally been re-written, but their forecasts now started from a different place. On the other hand, it should be noted that, viewed another way, the revised data also 'explained' why their February forecasts were generally too low - they had been based on inaccurate information.

Using the October meeting this chapter examines three main themes. The first theme is the October Report itself and the changes which have been made to the forecasts since July. A particularly important part of this discussion is the way in which the data revisions are used by the members of the Panel.

The data revisions are also important in the second theme to be discussed, namely the naturalisation/normalisation of ambiguity through consensual accounts. In the July Report, the unemployment figures were something of an anomaly and Chapter 6 examined the range of explanations which were on offer. By October, data from other parts of the economy had allowed the majority of the Panel to converge on an

explanation in which the unexpected falls were understood as a rapid rehiring of workers, itself made necessary by excessive job-shedding in late 1992.

However, as one uncertainty is resolved, so a new one invariably emerges - in this case the interpretation of the monetary indicators, the cornerstone of monetarist economics. In contrast to the discussion of unemployment, about which some sort of closure appears to be occurring, the discussion of monetary indicators illustrates the difficulty of resolving one of the longer running debates in recent macroeconomics.

Finally, the chapter examines the policy recommendations made by the Panel and compares them with the policies subsequently announced by the Chancellor at the end of November. The effectiveness of the Panel in influencing policy was discussed briefly at the end of Chapter 6. In that chapter the discussion focused on the medium term development of economic policy. In this chapter, the focus of the analysis is on the short term and examines two main issues. Firstly, it examines the nature of the consensus on policy which the Panel appeared to reach despite their different theories, models and forecasts. It is suggested that the consensus is, at least in part, due to the efforts of some of the Panel members to be seen to achieve it. A second factor is the relatively non-doctrinaire nature of the problem. The second part of the analysis focuses on the way in which the economically non-controversial reasons used by the Panel to support their recommendations are at odds with the political calculations the Chancellor must make. In conclusion, it is suggested that one reason why the economists, like many other scientific advisers, are not particularly good at setting the policy agenda is that their analysis does not address the problems as they are perceived by the politicians.

October 1993 Report

Introduction

The Panel of Independent Forecasters met for the third time in October 1993. As the year was nearly over, an increasingly clear consensus was beginning to emerge about what the final outturns for 1993 would be. This is not particularly surprising, after all, there were only 10-12 weeks left to go and most of the year's economic activity was already 'in the bag', so to speak. Nevertheless, differences did remain between the Panel over what to make of events so far. In particular, those most pessimistic at the beginning of the year, still tended to see the recovery as weak and fragile, while those initially optimistic, saw it as basically assured. As ever then, the consensus on the surface hid a range of views underneath. The rest of this section outlines the main

features of this consensus, together with the uncertainties and assumptions which underpin it and the range of views available.

The Panel introduced their October Report as follows:

> The UK economy has been growing for the last year and a half, according to the latest data. But the weak private sector balance sheet position and the European recession mean that we have yet to see growth above the trend rate of 2-2.5 per cent. While all of us expect higher growth in 1994, this rests on an assumed cut in interest rates, a projected fall in the savings ratio, and a strong turn-around in stockbuilding. None of these is guaranteed.[2]

The Panel's verdict on the economy in 1993 was not therefore particularly encouraging. Although positive economic growth had been recorded during 1993, the rate of growth had yet to reach, let alone exceed, the trend level of about 2.5 per cent per annum[3]. In addition, the future economic growth which the Panel had forecast was conditional on several important changes in economic policy and behaviour - none of which might actually happen.

As a result of this likelihood that economic growth would remain weak, and might actually get weaker, the Panel was concerned that the fiscal tightening introduced in the March Budget might not be sufficient to bring the PSBR under control sufficiently quickly. The policy problem, as they saw it, was how to raise revenues (or reduce expenditure) without killing off what little recovery there was. On a more positive note (for the government, if not for those that remained under- or un-employed), the weakness of demand implied that inflationary pressures would remain weak and that their inflation target was unlikely to be breached.

Forecasts for GDP and Demand

In October the consensus view of the Panel was that, according to the revised data (of which, more later) economic growth had turned positive in the second quarter of 1992 (i.e. *before* the exit from the ERM which had led to the inauguration of the Panel) and that the recovery was thus about 18 months old. In addition to putting the start of the recovery back to 1992, the GDP growth figures for the first half of 1993 had also been revised upwards and the composition of demand changed. In particular, the trade data, which were now collected under a new VAT-based system showed big falls in the levels of both import and exports. However, net trade (the difference between the exports and imports) had been revised upwards quite dramatically. Finally, manufacturing output

had been revised downwards, thus resolving one of the anomalies from July when the figures seemed rather high. As a result, the composition of demand was now much more like the export-led growth forecast by Congdon and Minford in February than the domestic-led growth the rest of the Panel had forecast at the same time.

Data revisions are by no means unusual in economics. However, what made these ones particularly difficult to interpret in the short time between the release of the data in September and the Panel's meeting in October was that they combined several different sorts of change simultaneously. David Currie explained the differences between the data available in July and that released in September, together with the difficulties the changes posed, as follows:

> *Currie* The latest data includes not just the change in the base year and the base weights but also revisions to the data which would have occurred if the data were still at 1985 prices. For example, manufacturing output has been revised down because the CSO has deflated part of manufacturing output (that destined for exports) by the export price deflator. This has risen by more than domestic prices in the last year following the devaluation so that overall manufacturing is less buoyant than before. The fact that the latest data encompasses two distinct revision procedures creates additional difficulties for our 1985-price based forecast.[4]

Currie also provided a table which illustrated the differences between the two data sets:

Table 1: Differences between 1985 based data and 1990 based data[5]

	annual per cent change			
	1990 prices		1985 prices	
1993	**q1**	**q2**	**q1**	**q2**
GDP	1.4	2.0	0.9	1.5
Consumption	1.9	1.6	2.3	1.9
Investment	0.9	0.3	1.4	1.6
Government consumption	-1.6	-0.3	-2.6	-2.7
Exports	1.9	-0.4	n.a.	n.a.
Imports	0.2	-4.8	n.a.	n.a.

As can be seen from Table 1, the effect of the changes was to increase recorded GDP growth in the first half 1993 by about 0.5%. Although not clear from the Table, the source of this extra growth must be from improved net trade as most of the other categories of expenditure have been revised downwards from their previously recorded values. For example, consumption growth is about 0.3% lower, investment 1.3 % lower

by the end of the second quarter. The growth areas are government consumption (in the sense that it has not been reduced by as much as was previously thought) and the unspecified net trade (for which comparisons are impossible due to the revised data collection methodology).

As a result of these changes, the majority of the Panel had revised their July forecasts up. For example, Tim Congdon wrote:

> *Congdon* GDP now seems likely to grow in 1993 by between 1.5 and 2 per cent. This figure is rather higher than forecast in Lombard Street Research's February and July Submissions. However, the growth of output during 1993 itself has not been much different from that envisaged in those two Submissions. The previous two forecasts were too low largely because the official statisticians have altered the quarterly profile of growth in 1992. (The effect of changing the profile was to alter the relationship between the level of output in 1992 on average and in the fourth quarter. Since output in 1993 starts from 1992's fourth quarter, the relationship between the average of 1992 and the average of 1993 is affected.)[6]

One thing to note about this interpretation is the way in which it maintains the validity and integrity of Congdon's theoretical framework and forecasting skills - the mistake is with the data collectors, not the model or the modeller. Several other panel members make essentially similar points. However, there is a notable exception to the general upward revision of demand forecasts, and that is Andrew Britton. In Chapter 5 it was pointed out that the National Institute's forecast for GDP growth of 2% in 1993 was initially an outlier, being more optimistic than the rest. Since February, the other forecasts had drifted upwards towards the National Institute figure. However, the 2.0% growth forecast by the National Institute was produced using the original 1985 based data for GDP growth in 1992. All other things being equal, one might expect that the National Institute forecast would have increased with the new data (for the reasons given in the extract from Congdon's submission quoted above). However, Andrew Britton interprets the data revisions in a rather different way to Congdon.

> *Britton* The numbers attached to this submission make use of the new 1990-based National Accounts data, but they should be regarded as a 'provisional' update, as we have not yet had time to rebase the whole model. The level of GDP in the first half of the year now appears a trifle higher than we thought when we prepared our forecast for the July report of the Panel ... So far as

this year is concerned we are standing by our forecast of 2 per cent growth, noting with satisfaction the way in which revisions to the data confirm our judgement of the strength of the recovery earlier this year. Several other forecasts have been revised up towards that figure, so we no longer appear an outlier.[7]

There are several interesting points to note here. The first is that the data revisions are not particularly significant - GDP in the first half of 1993 is 'a trifle higher' than previously thought. Whether Britton is referring to difference between the National Institute's estimates for GDP in the first half of the year or the provisional data released by the authorities is unclear. Nevertheless, the clear implication is that the changes to the data are not significant enough to warrant a change of forecast. In other words, the step change perceived by Congdon and the rest of the Panel is not recognised as such by Britton. Rather, it is as if a fog has lifted and the statisticians can now see the economic growth the National Institute had divined 12 months previously.

However, the fact that the National Institute's forecast has not changed surely implies that something else - their model, their theory or their judgement - *has* changed. The inputs to the model (i.e. the data for 1992) have changed and yet the output (i.e. the forecast for 1993) has remained the same. In other words, what we seem to be here seeing is a downward revision to the National Institute forecast towards the new consensus value of about 2%. One implication of this is that the model was incorrectly 'calibrated' at the end of 1992, and forecast a bigger impact from the devaluation than was eventually recorded.

Finally, a third reaction to the data revisions is also possible and that is simply not to trust them. For example, Gavyn Davies forecast that economic growth would be subdued principally because net trade would remain weak due to too-high UK interest rates (in February he recommended that they should be cut, although he had changed his mind on this by July) and because of low economic growth in Europe. Davies focuses his discussion on the data relating to net trade and stockbuilding which he finds rather implausible. The result is that although his October forecast for GDP growth looks pretty much the same as the one produced in July, its composition has changed to something which Davies cannot quite bring himself to trust:

Davies The rebased GDP statistics suggest that stocks have continued to decline at a pace equivalent to about 1 per cent of GDP per quarter, which is more than I expected in July. Mainly as a result of this, the current forecast shows

domestic demand rising by only 1 per cent in 1993, as against 2 per cent last time. However, the impact of this on GDP is more than offset by the surprising weakness in import volume, which the GDP statistics suggest dropped by 2.4 per cent in the first half of this year. As a result of this drop in imports, net trade is significantly more robust than expected last time, and this produces a modest upward revision in the GDP forecast for this year from 1.7 per cent to 2.0 per cent.

Davies In general, I would say that the GDP figures for the first half of 1993 look a little odd, and it would not be surprising to see subsequent revisions showing more stocks and more imports. Therefore, the expenditure components in the forecast are subject to this uncertainty.

Like Congdon, Davies recognises that the new numbers are significantly different from the ones they superseded. However, he does not accept, as Congdon appears to do, that the numbers are actually reliable. In essence, Davies asserts that his analysis remains correct and although, it is not currently supported by the data, it will be in due course. As noted above, the new figures do change the composition of demand to make it much more like that forecast by Congdon, and rather different from that expected by Davies. Thus, for Congdon, the data revisions affirm what he expected, whereas for Davies they challenge his forecast and need to be neutralised if the integrity of his own analysis is to be maintained. Although not disregarding the figures completely, Davies clearly feels that he would like to see a bit more evidence before he can accept the new data unreservedly.

More generally, it can be noted that the changes to the domestic data do not really affect the interpretation of the world economy. As a result of this, and despite the increases in domestic GDP, the majority of the Panel continue to hold their original views about the prospects for growth in the short and medium terms. This is implicit in the extract from Congdon quoted above: 'the growth of output during 1993 itself has not been much different from that envisaged'[8]. The same point is made by David Currie: 'The prospects for short term growth have changed rather little since our last report in June.'[9] and Andrew Sentance: 'Since the Panel's last report, the evidence from official statistics and CBI surveys has continued to support the view that a slow and steady recovery is underway.'[10]

Inflation

The forecasts for inflation are generally little changed from their July levels, with the average being 3.2 per cent. The highest forecast for the government's target measure, RPIX was produced by Andrew Britton (3.6 per cent), the lowest by Tim Congdon (2.8 per cent). Of course, as pointed in out in Chapter 6, the fact that the inflation forecasts have not changed even though the GDP growth forecasts have does imply that, once again, the output-inflation trade off has changed. In October, the UK can now get even more growth for the same (perhaps less) inflation than it could in July or February. This aspect of the apparent non-change in inflation forecasts was discussed in Chapter 6 and the arguments will not be repeated here.

There is however one interesting thing about the inflation forecasts in October and that is that just over a year has passed since the devaluation and exit from the ERM. If past economic patterns repeat themselves, inflationary pressures should, according to the mainstream view, be increasing as a result of the increases in import prices. However, this does not appear to have happened yet and there remains a clear bifurcation of future views over this issue. On the one hand there is the majority of the Panel which expects inflationary pressures to pick up as the recovery gains pace. On the other, there are those who think that inflation will fall still further and remain low.

As discussed in Chapter 5, the Panel members who are predicting that inflation is likely to rise are basically arguing that the economic experiences of the past will repeat themselves. They are thus arguing that the current situation can be understood as a 'typical' devaluation in which the gain in competitiveness due to the lower exchange rate is eroded as domestic prices rise.[11] Of course, things are not quite that simple as history never repeats itself exactly. In the context of 1993, this means that those economists who are arguing for the 'typicality' of the devaluation need to explain why, in fact, inflation has fallen throughout 1993. To do this they must adduce additional 'special' factors which can be inserted into the 'typical' devaluation story in order to explain how the general case applies to this specific instance.

The highest forecast for inflation, in both 1993 and 1994 comes from Andrew Britton and the National Institute. Although the National Institute's forecast has been reduced from 4.0% in July to 3.5% in October, Britton is keen to minimise the change, again attempting to emphasise how the outturn remains consistent with the original forecast. In this case, the apparent significance of the forecast revision is attributed to the essentially arbitrary inflation target chosen by the government - the July forecast

was above the target, the October one is below. In other words, although *politically* significant, the change is *economically* unimportant.

Nevertheless, the revised figure for inflation is lower than the National Institute's analysis of the 1967 devaluation had led them to forecast in February. What then are the special factors which have intervened in order to bring about lower than expected inflation? According to the National Institute one of the reasons for the recent low inflation figures is the large discounts being offered in the 'summer sales'. Clearly this effect is temporary (unless repeated as Autumn and then January sales) and the typical pattern of rising growth leading to increasing inflationary pressure is set to reassert itself during 1994, with additional pressure coming from the indirect taxes announced in the March Budget (itself perhaps another unusual or atypical event).

Britton The latest inflation figures confirm that price cutting at the time of the summer sales was an important factor in some of the very low figures recorded earlier this year. The trend of the series from now onwards is likely to be gently upwards if the recovery of output continues as we expect. This gradual inching upwards will be continued next year, reinforced by the effect of increases in indirect taxation. Allowing for the average margin of error the underlying rate of inflation at the end of next year will probably be in the range 3 to 6 per cent.[12]

The other mainstream economists on the Panel also told a similar story for the future - rising inflationary pressures in the year ahead - although they used somewhat different factors to explain why the general case had manifested itself in a rather unusual fashion on this particular instance. For example, David Currie attributes the unexpected low inflation to external events in the world economy. However, Currie's central story remains that, as the devaluation and tax increases work through, inflation will increase in the coming years:

Currie Recent favourable trends, including subdued world inflation and continued falls in manufacturing unit costs, make us more optimistic about inflation than we were in June: we see the Government's preferred inflation target (RPI ex mips) averaging just over 3.5 per cent next year and in 1995. But we think that it is quite likely that inflation will breach the 4 per cent ceiling during the next year, as the impact of last September's devaluation and higher indirect taxes feed through to prices.[13]

Gavyn Davies tells an essentially similar story in which price rises due to the devaluation should soon be reflected in the inflation figures. Like Britton, he identifies rises in goods prices as a key upward influence, but notes also that wage and service inflation are low. However, Davies seems to see the devaluation as working more or less normally. Goods and food prices have risen sharply, but the expected increases in the aggregate indices have been minimal due to the unusually low rate of wage inflation which has offset the rises in other prices:

> *Davies* It is true that services inflation is continuing to drop, and that wage pressures remain notable by their absence, but food and other goods price inflation has been rising sharply since last year's devaluation. Provided that the exchange rate remains broadly stable in the next 6-12 months, these devaluation effects should be fully reflected in retail prices reasonably soon, and the Goldman Sachs inflation model continues to see core inflation - excluding mortgages and all tax effects - falling from about 3.5 per cent now to about 3 per cent by the end of next year. This, however, would be consistent with the headline inflation rate rising from 1.7 per cent now to about 3.7 per cent next spring as mortgage rate cuts pass through the index. In addition, the government's chosen target variable (the RPI excluding mortgages) may temporarily breach the 4 per cent target ceiling next spring before dropping back to about 3.6 per cent at the year end. [14]

Thus, Davies interprets the data in terms of a 'typical' devaluation story and sees the unusually low levels of wage and service inflation as partly offsetting the expected inflationary consequences. However, on balance, Davies is not as concerned about inflation as Britton. In contrast, David Currie identifies the current low level of wage inflation as a source of concern for the future. An important part of the traditional devaluation-leads-to-inflation story, is the notion of the wage-price spiral, which refers to the tendency of wages to rise in response to inflation, thus increasing prices still further.[15] This has yet to happen in the year since the devaluation. If the general devaluation explanation is to maintained this omission will have to be corrected eventually. Thus:

> *Currie* There is also the danger that the inevitable sharp rise in headline RPI inflation numbers next year from their current very low levels (with the lowest point already past) will combine with falling employment to push wage settlements higher.[16]

Britton and Davies to a lesser extent, although not highlighting this specifically in their reports, probably also share these concerns. However, not all the economists subscribe to this point of view. In particular the monetarist members of the Panel see the influence of the high unemployment rate exerting a significant downward pressure on inflation for some time to come. For Congdon, this is very much a business-as-usual interpretation in which unemployment above the natural rate (combined with a large and negative output gap) is associated with weak inflation.

> *Congdon* There has indeed been a strong impact from the devaluation on profitability and relative prices. But underlying inflation pressures have remained weak, largely because of high unemployment and abundant spare capacity. The GDP deflator went up by a mere 0.75 per cent (i.e. an annualised rate of 1.5 per cent) between the fourth quarter of 1992 and the second quarter of 1993.[17]

Unlike the mainstream forecasters Congdon does not need to introduce any special factors in order to justify his analytic framework. For him the current situation is a clear example of the more general case: 'high-unemployment-plus-large-negative-output-gap-equals-weak-inflation'. In addition to making clear the way in which the outturn is perfectly consistent with his theory, Congdon further reinforces his point by introducing survey results which show that decision makers in the real world are acting in ways consistent with his analysis.

> *Congdon* Particularly good news has emerged in the August and September CBI surveys, both of which reported more companies expecting to cut prices in the next few months than to raise them. Since the CBI survey relates to manufacturing and manufacturing output ought to respond to devaluation sooner than other sectors, the message seems to be that the inflationary impact of devaluation has been largely transmitted to the price level. This should not be a surprise, because - by common consent - the devaluation occurred from an exchange rate which significantly over-valued the pound.[18]

Like the rest of the Panel, Minford also sees that low inflation has come about due to the rises in some prices being offset by falling wage costs. In fact, this much seems to be shared by everyone.

Minford Some facts are now clear. Inflation has fallen and remained low (in spite of August's wobble), with the effect of rising traded goods prices more than offset by falling wage costs.[19]

The differences between the Panel are not therefore due to different data, but to different interpretations of the same data. The most radical interpretation of this data comes from Minford who sees in the current economic climate a gestalt switch into a completely different world. In this new, competitive world environment inflation is not just down for a few years (or however long it takes) until unemployment falls to the natural rate or growth accelerates - inflation is down and out for good:

Minford It is hard to escape the conclusion that, in an as yet slow, quiet, but still steady way, the same forces that transferred people in their millions off the land in 18th and 19th century Britain are affecting the fortunes of the world's richest nations: the OECD. Competition is growing inexorably and increasingly rapidly for the products in which they have had a commanding lead - manufacturing and other sophisticated industrial products. Under the influence of the computer and information industry revolution, technologies which once took decades to transfer to poorer countries can now be instantly transferred through multinationals or even, though more rarely, mere imitation by local companies. Poor, often ill-educated workforces can be harnessed to producing their products by use of computer-driven procedures which eliminate human error. Imported, those products displace their equivalents produced under hugely higher labour costs in the richer countries, just as the more cheaply produced and increasingly cheaply transported food from the empire and elsewhere displaced British-grown food after the repeal of the Corn Laws.[20]

The quotation is interesting for several reasons, not least of which is the social theories of knowledge and skill so casually imported. In Minford's world view, skills are unproblematically black boxed into technologies which are then taken to wherever the labour is cheapest - the story therefore is of technology de-skilling the labour force and of an inevitable techno-economic determinism. In terms of the Panel's brief however what matters are the implications of this industrial and manufacturing trend for the UK's economy and policy. Minford's view, as ever, is clear and concise:

Minford The policy implications for a small open economy like the UK are similarly blunt. This is an [international] environment so competitive that provided control of the money supply is exercised - a cliché today - inflation is dead

(given the fast growth of quality this may well mean 2 per cent inflation as measured); this is quite consistent with inflation remaining for a time over 3 per cent as traded goods prices reflect the fall in the pound.[21]

Of course, this view is not shared by everyone. For example, David Currie, one of the inflation pessimists on the Panel, flatly disagrees with the idea inflation is no longer an issue for economic policy:

Currie In this view, we differ from those who argue that the UK is heading for zero or very low inflation that will be sustained in the medium term. The sources of this difference are twofold: first, we are sceptical about the supposed greater flexibility of the UK labour market; and second, we are less optimistic than others about the durability of the recent remarkable productivity performance in UK industry.[22]

However, what is interesting about Minford's analysis is not that it is controversial but that, unlike the others on the Panel, Minford sees in the data on world growth (which is slow, especially for the traditionally powerful OECD nations) an emerging 'new' world order. Minford is thus saying that the changes in the world economy are such that economic activity in 1993 is different from the economic activity in the past and that the old patterns will not repeat themselves. The key factors lying behind this change are, internationally, the increasing mobility of global capital and, domestically, the changes in the UK economy which occurred during the 1980s.

Minford Ironically, but of course logically, the only countries which may be able to except themselves from this general OECD slowdown are those which have much technological catching-up to do, whose wages are low and whose environments are favourable to capitalism through deregulation: examples are Britain, New Zealand and possibly Australia, all of which recently emerged from a protected and inefficient past with resulting low wages and are resolved to reduce regulation in a bid to attract new investment.[23]

Thus, Minford can argue that although the UK has a low wage economy, high unemployment and a greatly reduced indigenous manufacturing base this, in the current context, is a good thing - the reason being that the UK is now able to be like the successful and fast growing economies of Asia and Latin America. In other words, the implicit social order, in which 'developing' countries and the Third World are assumed to want to be like the Western nations, is turned on its head. In Minford's world, if the OECD nations want to maintain their economies then they will have to adopt the

policies of the so-called 'Asian Tigers' like Taiwan and Korea. Consequently, it is the OECD nations who have undone the most post-war social legislation which have the best chances. This puts the UK in a good position relative to its European neighbours and competitors:

> *Minford* There are many more players now who have cottoned on to how to be an emerging market economy. It used to be just Singapore, Hong Kong and Taiwan, but now they are all playing the game - Indonesia, Thailand, India - it is just astounding what is happening. The whole of Latin America seems to be getting in on the act. Everybody has cottoned on to how [to be] attractive ... It just so happens that because we have been so backward, our real wages are quite low, and that means that if we can get to state of the art productivity ... we [can be] competitive relative to Europe for a whole lot of industries which have to be in Europe for the moment. I think that is a key window for us.[24]

Most of the Panel accept the theory of Comparative Advantage which lies behind Minford's argument. Comparative Advantage is a basic economic concept found in all textbook discussions of international trade and which states that nations will tend to concentrate their resources in the producing things which they make the best and not waste resources producing goods they can import more cheaply from elsewhere. In Minford's world the things which the UK is particularly good at making are cars and their associated components as well as consumer electronics like computers:

> *Minford* For example, we have got a tremendous opportunity in the car industry. It's clear that, largely because of a history of protectionism, there is going to be a European car industry for some time to come. But, with the entry of new techniques and the Japanese entry behind the European barrier, the shape of it is going to be drastically changed, and changed in our favour.[25]

Although Minford's basic point, that comparative advantage means that transferable technologies and industries will tend to move out of the OECD countries, is accepted by the Panel, his conclusions about the speed and size of the effects are more contentious. In particular, the rest of the Panel doubt that it is a widely or as clearly applicable as he claims. For example, Gavyn Davies says:

> *Davies* I'm not sure I go the whole hog with Patrick. I think he may have put his finger on an interesting idea, but I don't feel that I have thought about the magnitude of these flows sufficiently to know how important it is ... I think,

in principle, Britain does have a slightly more flexible labour market than Germany, France, Spain and Italy, and I think that in responding to shocks there may be some advantage in having such a flexible labour market. But it is not obvious.[26]

Similarly, Andrew Sentance can see the economic forces to which Minford is referring and, whilst acknowledging the quality of the question, is not so sure that Minford's answer is correct:

Sentance I feel that he always sort of overdoes this optimistic [interpretation]. I mean you could present it in a different way. Another implication of what Patrick is saying is that Europe is going to have a pretty tough time in the 90s and that that is going to drag us down. I would go some way along that argument with him, but I would say that it is not all beneficial for the UK.[27]

Thus, Sentance sees that Britain might be unable to exploit what comparative advantage it may have because the European economies, as a result of their more global comparative *dis*advantage, remain too weak to import UK goods, even if they are competitively priced. Moreover, Sentance disagrees with the conclusions which Minford draws for policy. Minford, as can be seen from the quotation given above, believes that there is a recipe for success in the current economic environment - Hong Kong and Taiwan had it first, but now Indonesia, Thailand, India and the whole of Latin America have 'cottoned on' to how to do it. Minford's policy prescription is thus that European economies must adopt the same sorts of economic policies as those found in the emerging economies. However, it is not at all clear that this necessarily follows from the analysis. The appropriate policy response, as Sentance explains, is therefore a difficult and important question:

Sentance If you accept Patrick's story, that there is some sort of revolution going on which is creating growth elsewhere in the world to which we have to respond, what is the appropriate economic policy response? Does it mean, for example, that we are going to have a continued deflationary pressure on our economy because of all this cheap competition, and therefore monetary policy needs to remain pretty relaxed? ... Or is the main implication that we have got to be more like these other economies in terms of the size of our public sector taxes? [That] is one implication that Patrick draws ... [but] if you talk to people on Continental Europe about this, the sort of conclusion that Patrick draws is anathema to them ... They say that we [Europe] can't compete with these countries in the Third World. There is no way.

> Therefore, we have to move onto the next stage in terms of more technological sophistication and so on and so forth.[28]

To sum up this discussion on comparative advantage and inflation, we can see that the middle view is that the slow growth of the OECD economies, particularly the European ones means that the UK's main export market is likely to be weak and that exports will have to remain competitively priced if they are to sell. As a result of this inflation is likely to remain low (but so will growth, and big falls in unemployment are unlikely). Those who forecast higher inflation tend to minimise the differences between the previous experiences of the devaluation and the current one, attributing the current unexpectedly low inflation to unusually aggressive summer sales and surprisingly weak wage inflation. These temporary factors delay the inflationary impact of the devaluation, but eventually, the typical pattern will re-assert itself as growth and employment pick up leading to increasing wage and hence inflationary pressures. Congdon is also in this camp, although his theoretical background is different and his forecasts for inflation lower. Nevertheless, his analysis implies that nothing significant has changed in either the world or domestic economies. Finally Minford sees the potential for a dramatic improvement in economic output. Unshackled from the inefficient past of its indigenous industries and with low wages and plenty of unemployed workers, Britain is Europe's best place to invest. Moreover, because, in Minford's world, international competition is so tough, inflation is simply not possible anymore.

Unemployment

Unemployment was discussed extensively in the July meeting and this discussion formed a substantial part of Chapter 6. In July, the falling unemployment figures recorded in the early months of 1993 had caused come difficulties for the Panel and a variety of explanations for them were on offer. By the October meeting, the falls in unemployment had stopped and the Panel were all forecasting that unemployment levels would remains fairly stable for the rest of the year before falling slightly during 1994. In addition to the levelling off of the unemployment figures, the passage of time had also brought about a closure of the debate over the causes of unexpected falls in unemployment which occurred in the first half of 1993. The collective view of the Panel over the cause of these falls was as follows:

> Developments in the labour market now look much less odd than they did in July. Then we saw fairly large falls in **unemployment** virtually coincident

> with the upturn in activity in the first quarter of 1993. But unemployment has since levelled off, and the profile of non-oil GDP has been revised to show it rising steadily from three quarters earlier than before.[29]

As can be seen from this quotation, the unemployment data, which were so puzzling in July are, by October, well understood, with a consensually accepted explanation. Part of the solution to the controversy lies in the new statistics. In Chapter 6, it was pointed out that part of what made the interpretation of the unemployment figures difficult was that the falls had occurred much earlier in the economic cycle than would usually have been the case. In terms of the analytic categories deployed here, the difficulty was how to explain the current situation as an example of the general case. With the new data, the start of the economic recovery has been moved further back in time, and the falls in unemployment now occur later in the economic cycle. In this way the particular case is now more easily understood in terms of the general pattern.

However, there is a slight complication. Although falls in unemployment are now to be expected in the early part of 1993 (as a result of the recovery) the rapidity of these falls still needs to be explained. It turns out that the explanation for this event, which does not fit the general pattern, still requires some special or unusual factor to explain it. The reason for the rapid falls in unemployment which the Panel endorses is that the *falls* were not the aberration. Rather, the aberration was the *rise* in unemployment which took place in the months before Christmas 1992. During this period, when it must be remembered there were no economic data to suggest recovery, the Panel believes that there was:

> ... a collective loss of business confidence ... which resulted in many firms either bringing forward labour shedding or delaying hiring. This lead to fewer redundancies, and more hirings, in early 1993, and consequently a fall in unemployment.[30]

Thus, the changes in the economic data enable the economists to normalise the puzzling unemployment data by re-asserting the link between the economic cycle and the unemployment data. However, the rapidity of the early falls needs to be accounted for, and this is done by attributing the deviation from the general case to a loss of confidence in the business community. This then causes unemployment to rise above what it should 'really' be. In 1993 the 'mistake' is then corrected and the unemployment data is returned to its more usual relationship with output.

As a result of the changes in the data, and the subsequent naturalisation of unemployment, those economists who had introduced special factors like the behaviour of the DSS into their analysis had to re-assess these ideas. For those who did not agree with this proposed explanation, the new data clearly reinforce their original scepticism and confirm that the others were simply wrong. For example, Gavyn Davies clearly highlights the way in which the data are now perfectly consistent with the general story and that nothing else is needed:

> *Davies* The Panel debated the unemployment statistics at length last time. Since July, the Labour Force Survey has been published and this confirmed that there was a drop in unemployment on the ILO definition of 61,000 between Winter 1992 and Spring 1993. Over the same period there was a rise in employment of 151,000, so the relationship between the change in employment and unemployment was not particularly unusual. This does not lend support to those who believed that the drop in unemployment earlier this year was due to administrative changes.[31]

As can be seen, the unemployment data is now uncontroversial and alternative accounts are marginalised. The extent of the marginalisation of alternative accounts can be seen in the Submission of Andrew Britton. Britton was one of those most clearly associated with the idea that the falls were the outcome of the administrative processes, rather than economic activity. By October he rejects this hypothesis but does so in a casual reference to the Spring Labour Force Survey, in a paragraph ostensibly dealing with the revisions to the National Accounts and the outlook for demand:

> *Britton* One indicator that we find particularly encouraging is the Spring Labour Force Survey, which confirmed that the fall in unemployment was not confined to benefit claimants. It also indicated a substantial rise in employment, suggesting that activity may have been increasing quite substantially at the time.[32]

Of course, Britton was not wrong in any significant or pejorative sense - in July no-one knew what was happening to the unemployment data. He simply recognised that something odd was going on and offered an explanation. Now, 3 months later, what was originally odd no longer appears so, his hypothesis is no longer needed, and the economic orthodoxy is restored. The majority of the Panel seem to accept this explanation, but there are some outliers. Ever the mavericks, Wynne Godley and Patrick Minford disagree both with the mainstream and each other.

Firstly, Wynne Godley is keen to stress that the current situation is perfectly consistent with his own theoretical view point according to which the UK's economic problems stem from a long-term decline in net exports. In fact, Godley claims that the 1993 situation is 'well in line with the strategic predictions which [he] was making, with Cambridge colleagues, throughout the seventies and early eighties, although the violent fluctuations which have occurred have tended to distract attention from the underlying trends'.[33] With regard to the unemployment figures, Godley, like the others, sees the rise in unemployment in the Autumn of 1992 as an 'aberration' which has been, to some extent corrected. However, unlike the others, Godley does not accept that the contribution of the administration of benefits to the claimant count measure of unemployment can be dismissed so lightly.

> *Godley* [One explanation for the fall in claimant count unemployment] derives from the fact that the claimant count is, in the words on the *Employment Gazette*, 'a by-product of the administrative system used for paying benefits'. It seems clear that people who cease to be employed may find themselves inside or outside the categories which make them eligible for arbitrary reasons ... there are reasonable grounds for supposing that, of those people who have lost their jobs over the last three years (including many self-employed), a greater proportion than normal have left the labour force and therefore that unemployment, properly understood (that is, the number of people who are not employed and are seeking jobs) is higher than the number successfully claiming unemployment benefit. This picture is confirmed by the fact that employment has fallen rapidly over the last year and has hardly increased at all during the last few months, while the population of working age has continued to increase.[34]

Unlike Davies and Britton, Godley does not interpret the data in such a way as to reject the idea that the benefit system has no effect on the level of claimant count unemployment. Also, and again unlike Britton, Godley is unimpressed by the increases in employment recorded in the Spring Labour Force Survey. Godley's approach is to increase the complexity of the analysis and thus to deconstruct the simple relationships between unemployment and employment which economists like Davies aimed to restore. For example, 'unemployment' is problematised by Godley to include all those 'not employed and seeking work' whilst the background assumptions about the size of the labour force are also highlighted. These points are not addressed by the other Panel members in their Submissions, so Godley's arguments remain marginalised. This

situation is no doubt reinforced by the willingness of Britton to drop the idea he originally backed.

Finally, Patrick Minford also doubts that everything is quite as normal as the mainstream Panel members make out. Minford believes that unemployment has 'fallen rather earlier in the cycle than in previous upturns', and interprets this as an indication that 'labour market behaviour is changing'[35]. As discussed extensively in the previous section, Minford believes that the economic climate in the 1990s is very different to previous economic cycles. In addition, Minford ardently believes that the UK labour market is now much more flexible than in previous economic cycles and so he might well expect unemployment to fall more quickly.

In Minford's frame of reference, this change in labour market behaviour is not a peculiarity which needs to be explained away, but rather an indication that policies have worked, i.e. it is something to be celebrated. Indeed, the falls in unemployment form one of an agglomeration of facts which Minford introduces to show how the economy has confounded the 'Supply-Side Pessimists' who think its plight is caused by structural factors like the inflexibility of markets and the poor competitiveness of industry. In other words, for Minford's general analysis to hold, labour market behaviour *ought* to have changed (or at least be changing). This is different from the mainstream economists, who generally doubt the efficacy of the policies Minford has championed, and for whom the past patterns ought to recur if their general case is to be supported.

To sum up, we can see that there has been a general move to normalise the previously problematic data. In particular the debate has closed around a consensus view according to which the unusual event was a collective loss of business confidence in the Autumn of 1992. This led to more redundancies and less employment than was appropriate. In the early months of 1993, the 'mistakes' of 1992 were corrected and unemployment returned to roughly where it ought to have been and the Panel members returned to their general stories and forecasts. However, in the background there remains Wynne Godley questioning the definition of unemployment around which the debate took place and thus highlighting one of the taken-for-granted assumptions of the rest of the Panel.

At this stage in the upswing of the economic cycle it is really too early to assess which how the debate about unemployment in the longer term will work out. In October 1993 all parties to the debate could see something in the data to support their

views. The rest of this section uses a discussion of monetary indicators to show how it is unlikely that the simple accumulation of more data will enable the economists to agree on a theory of unemployment.

Monetary Indicators

The importance of monetary indicators for understanding the economy has a long and controversial history in economics. The aim of this section is not to rehearse the arguments of the past[36], but simply to show that they are still going on. The focus of the analysis in this section is as follows: in October 1993, money supply growth figures in the UK, if you believed them, were sending quite different signals depending on which definition of the money supply you used[37]. 'Narrow' money (M0) was growing at about 5 per cent, which implied GDP growth in the range of 2-3 per cent and no urgent need to relax monetary policy in order to boost economic growth. On the other hand, 'Broad' money (M4) was growing at about 3 per cent, implying virtually no growth in GDP at all, and consequently suggesting that the authorities needed to act in order to ensure the recovery was sustained.

The question I put to the economists was as follows: 'How would you interpret the figures recorded for money supply growth, if indeed you pay any attention to them at all?' In other words, what, if anything, does the growth of the money supply tell us about the economic future?

Narrow Money

It is probably most convenient to begin by outlining the views held by the mainstream members of the Panel. Generally speaking, economists who are not 'monetarists' do not see money as causing economic activity, but rather as reflecting it (although this is not as clearcut for M4 as it is for M0). If we consider M0, the view of mainstream non-monetarist economists is neatly expressed by Andrew Britton:

> *Britton* We don't see it as being a factor causing consumers' expenditure though, because essentially the banks provide the people with the cash they want.[38]

Interestingly, 'Broad Money Monetarists' like Tim Congdon are even more sceptical of its utility[39]:

> *Congdon* Well I basically don't take any notice of M0 and narrow money[40]

Given this, you might be forgiven for wondering why anybody bothered to collect information on M0 at all. One reason for its usefulness is that it is widely regarded as a

contemporaneous indicator of economic activity. Thus, Andrew Britton, whilst not seeing M0 as causing consumers expenditure, does include M0 amongst the list of economic data worth watching because it gives a 'useful guide to what is happening in the shops'[41]. However, M0 is not regarded as a leading indicator in the sense that knowing what the growth in M0 is will enable you to forecast, say, consumers' expenditure any better. Rather, the 'advantage of it as an indicator is that you can get figures on it which are quite up to date, more up to date than for the value of retail sales.'[42] Similarly Gavyn Davies summarises his position as:

> *Davies* Well my view is that you shouldn't rely on either of them [M0 or M4]. But M0 over many years has been a better contemporaneous indicator of activity than broad money. It may not give you a very substantial amount of lead time - I think M0 gives you a little bit of lead time, but not a hell of a lot of lead time - M4 doesn't give you any.[43]

Sticking with narrow money, Tim Congdon questions even the usefulness of this contemporaneous information:

> *Congdon* Well, yes, it is [a contemporaneous indicator] but you have got lots of other data, so why bother? And the things that it is indicative of are things that aren't actually very interesting. Retail sales are a function of income - they don't cause fluctuations in income.
>
> It is one of the most basic messages of Keynes' economics - income is a multiple of investment. It is not consumption is a function of income. If you want to understand fluctuations in economic activity, you don't look at retail sales, you look at things which really swing around like stockbuilding and capital spending.[44]

However, the mainstream economists view remains that:

> *Sentance* M0 has tended to have a fairly reliable relationship with the consumer spending side of the economy as a sort of contemporaneous indicator[45]

and Andrew Britton can thus conclude that it is 'giving a signal which is quite consistent with what we think is happening'[46]. In other words, the growth of M0 was consistent with consumers expenditure growing by 1.8 per cent in 1993.

However, the interpretation of M0 is not mechanical. Although the relationship is 'fairly reliable' it can, does and has changed over time. For example:

Sentance the velocity of [M0], the ratio we're spending appears to have shifted. It used to grow at roughly, I think, 4 per cent slower than the value of cash transactions, but it is growing much more in line with the value of cash transactions now.[47]

Thus, at all times the economists have to think about how the current circumstances affect the general relationship. From the example given by Andrew Sentance, we can see that the velocity of M0 has changed over time and now grows at the same rate as the value of cash transactions. This change implies that there is now a faster growth in the quantity of narrow money for a given growth in consumers' expenditure (and perhaps GDP) than would have been the case in the past. The task for the economists is how to explain these particular events as cases of their more general theories. In the case of narrow money this is only a problem for Patrick Minford, as he is the one Panel member to attach any great importance to M0 - for the rest, the instability of the relationship *is* the general story.

For Patrick Minford, the key thing which makes interpreting growth on M0 difficult is that the economic situation in 1993 is actually rather unusual and so it is not particularly surprising that the M0 relationship might have changed. In fact, Minford identifies two reasons why one would not expect M0 growth rates to conform to typical patterns. The first factor is that interest rates in 1993 were unusually low. Thus:

Minford I think that the M0 figures are consistent with a little lifting in consumer spending, but the difficulty with M0 of course, when you have got a period of low interest rates is ... the reliability of the relationship. Most of the estimated period has been when there have been quite high interest rates, and there's a modest interest elasticity.[48]

This is really just an argument from basic economic theory - as interest rates rise, so the opportunity cost of holding cash relative to holding it in interest bearing accounts increases and the amount of cash held should decline. When interest rates fall, so does the opportunity cost, and hence the amount of cash in circulation increases as a result of the desire to finance transactions speedily and conveniently. This argument thus implies that money supply growth would be higher at low interest rates.[49] However, this relationship between narrow money and consumers' expenditure is complicated by a further factor, identified first by the National Institute:

Minford ... there is a cumulative technological trend which seems to be quite heavily influenced by the accumulation of interest rates. In other words, you get [a]

faster technological [move] away from notes and coins when the interest rate is high, and it is irreversible. So what happens is that suppose you have two years of 15 per cent interest rates, that creates two steps in the technological shift away from M0 and once they have occurred they cannot be reversed

Evans So as interest rates become high [and] people want less cash, technology is developed which facilitates these kinds of [cashless] transactions?

Minford That's right, the technology comes along when interest rates are high to facilitate people not using cash, as it were, once and for all. That creates a shift downwards in the demand for cash, and then even if interest rates from then on fell off, it wouldn't go back. So its like a trend term. If one thinks of there being a normal growth of M0 demand due to growth in inflation, and thinks of this technological substitution as occurring steadily at high interest rates but not occurring at all at low interest rates, you could well get a different velocity trend when interest rates are low.[50]

The conclusions Minford draws from thus are that, at low interest rates, the growth in M0 will therefore be higher than at higher interest rates (for a given rate of growth and inflation) because:

Minford you've got to make an adjustment for the fact that there is now probably a faster trend growth of demand for M0, given that interest rates are low. [Plus], you've not just got the fact that interest rates are low increasing the demand for M0 once and for all, you've [also] got a steady tendency now for it grow faster, for a given level of inflation and growth, because before you had this steady technological substitution occurring at high interest rates and now you haven't got it.[51]

Thus, any mechanical reading of money supply growth figures will lead to mistakes:

Minford I think that for M0, one has probably got to think of the target range as being systematically too low at low interest rates. So while I think the M0 figures are consistent with modest growth of domestic demand, which is welcome, it is certainly not consistent with the sort of rapid growth in domestic demand that it signalled in say 1988, when it was growing at 5 per cent.[52]

Summary

From the above passages we can see how the controversy over the meaning of the quantity of narrow money can have continued for so long. For the mainstream economists, the quantity of narrow money reflects consumers' expenditure decisions and the aspects of economic activity related to them, such as retail sales. Cash is not seen to cause demand in any significant way, but the amount of cash held does provide some information which is useful, principally because it arrives promptly. M0 is therefore something which can be used to give an indication of what is happening currently and thus corroborates the stories, forecasts and expectations of the economists in a fairly general way. However, because it is not central to the economic story told by the mainstream economists a certain laxity prevails in the exactitude required of the relationship. Put another way, most economists think that they have more important things to worry about.

On the other hand, for a monetarist economist like Minford the same data series provides the basis for a complete story of economic action. In Minford's economics, 'monetary influences are key.'[53] and he skilfully weaves together a wide range of data, some of it generated by non-monetarist economists, in such a way as to understand the world as one in which the demand for money is systematically related to other aspects of the economy, particularly interest rates, prices and GDP growth. Clearly this intellectual work is put in by Minford because his economics requires that the relationship between the demand for money and other economic data be fully explained. However, Minford's economics is complex and subtle and incorporates a wide range of other factors in its analysis. Thus, although the core belief is clearly that monetary aggregates are important is clear in all Minford's economics, the exact monetary indicator used can vary and its interpretation is frequently a matter of skill and judgement. Thus, at the end of 1993, when, as noted above, the monetary indicators might have been seen as unreliable, Minford was prepared to concede the point, but not the theory:

> *Minford* Well I think that unfortunately the monetary indicators have become quite difficult to read just at the moment. I think that they do have some information, but the adjustment factor that has got to be applied to both [M0 and M4] is really quite difficult to judge. So you are really forced back onto other measures like real interest rates, savings ratio, the state of the economy ... I think that we were right to stress monetary aggregates, relative to the ERM, but of course now we have come out of the ERM,

> monetary aggregates are giving ambiguous signals ... it is just a period in which the thermometer is a bit wonky, but it will come back. The basic principle of using domestic indicators, and monetary indicators in particular, to guide monetary policy, I think is correct. It is simply at the moment that the monetary indicators have to be supplemented.[54]

As noted above, this is a concern not particularly felt by the rest of the Panel. That said however, the vast majority of the Panel did think about and pay some attention to growth in M0. Thus, although they do not see M0 as causing anything or as being as important in the same way that Minford does, the Panel generally regard M0 as giving some kind of information about the economy.

In this way, quite unlike the debates over unemployment which are still quite controversial, the debate over narrow money seems to bubble along gently with no-one relying solely on M0 and virtually no-one saying it is of no use. The exception is Tim Congdon. Like Minford, Congdon is a monetarist economist, but unlike Minford, who as we saw draws on a wide variety of monetary data, Congdon sees the sole monetary influence as being the broad money measure (M4). It is this reliance on a single indicator which gives Congdon his distinctive position. It is also what makes his economics harder for many of the other Panellists to accept.

Broad Money

In many respects the mainstream economists view M4 in much the same way as they view M0 - it reflects activity rather than causing it, although the relationship is not as clear. However, unlike narrow money, which, as we saw above, most economists would concede has a fairly reliable relationship with activity, and may even lead slightly, this is not seen as the case for M4. Thus for example, Gavyn Davies says:

> *Davies* [M0] may not give you a very substantial amount of lead time - I think M0 gives you a little bit of lead time, but not a hell of a lot of lead time - M4 doesn't give you any.[55]

Of course, this is most emphatically not the view of Tim Congdon, whose whole forecasting operation is based upon the premise that:

> *Congdon* movements in real broad money today give a lead to demand and output in the period six to 18 months away.'[56]

One thing which is interesting about this is the similarity in the backgrounds of the two economists which hold such different views. Congdon began his career as an

economics writer in *The Times*, and was influential in economic policy circles in the late 1970s and early years of the Thatcher administration. After this he worked as an economist in City firms before establishing his own economic forecasting consultancy, Lombard Street Research. Davies on the other hand has worked in both the Downing Street Policy Unit (from 1974 to 1979: the time when broad money targets first became important parts of UK economic policy) and three firms in the City of London (he is currently a Director of Goldman Sachs). However, the contrast between them could not be starker. Gavyn Davies describes his experiences, and the view to which they lead, as follows:

> *Davies* I have spent 20 years trying to fit equations which would explain price inflation or nominal GDP using broad money indicators and I've never found broad money helpful at all. I think it might be a climate indicator, but it is certainly not a weather forecasting indicator. It doesn't tell you about tomorrow's weather forecast.[57]

In other words, two views almost completely at odds with each other from two highly successful City economists - clearly 10 years economic data has done little to resolve the debates which were so prominent in the 1980s. But what are the sources of the difference?

It is clear that the source of Davies's disaffection is that the relationship between the monetary aggregate and economic activity does not produce a respectable regression equation - in other words, there is not a stable relationship between the two, unlike with narrow money. However, it is just this use of 'stability' as a criterion which Congdon objects to:

> *Congdon* It doesn't have to be stable. I mean, it could be unstable. It is a pity in a way that these words have become the arbiters of the whole thing. The demand to hold money balances can be very unstable. My point is simply that if the amount of money goes up by 50 per cent, or 10 per cent or 20 per cent, then whether [the demand to hold] it is stable or unstable, there will be profound effects on behaviour.[58]

Congdon's story is thus as much analytical as it is empirical. In fact in his very first Submission to the Panel he writes that 'changes in asset prices are, of course, related to - *though not mechanically determined by* - changes in broad money growth'[59] He explains the link between the quantity of money and economic activity as follows:

Congdon If the amount of money in the economy is increased by [government policy] it could have effects on personal sector money balances but, in fact, personal sector money balances are relatively stable, so it goes straight through into the corporate sector or the financial sector - and then you get the effects. In the case of the corporate sector on investment and stockbuilding. In the case of the financial sector, in the first instance on asset prices and, later on, on investment and stockbuilding [and] possibly on the exchange rate because they have too much money and they want to get it into foreign currency. At any rate, because of the change in the amount of money in the economy, you get powerful effects on asset prices and the volatile elements of GDP. That is the link between funding policy [i.e. quantity of broad money] and the economy.[60]

However, despite their different causal stories about broad money growth all the economists on the Panel track its progress. For Congdon, however, it is the key instrument of economic policy:

Congdon The question is, how do you define monetary policy? As far as I am concerned any monetarist has to define monetary policy in terms of the quantity of money. Now you then get on to the debate about whether the relevant aggregate is a narrow on or a broad one. I don't see how anyone can sensibly believe that narrow money causes everything, so the only thing that can be relevant from the point of view of determining behaviour is a broad monetary aggregate. So I, in all my work, would define monetary policy largely in terms of the behaviour of broad money. Well, the question then is, how is broad money affected by policy ... Well, as far as I am concerned it is largely banks' decisions and their customers' decisions about how much they wish to expand their balance sheets, particularly how much they want to lend. The two key influences are then how much banks can add to their claims on the private sector, i.e. lending to the private sector, and how much they can add to claims on the government, which obviously depends on their [i.e. the government's] monetary policy.[61]

For the rest of the Panel the story is not so simple - in fact it is extremely complex - and it is something which the Panel devote some considerable effort to resolving:

Britton when we [the NI] do our forecasts we don't actually make a forecast for M4, but along with a of number indicators we look at it and puzzle over its significance. And certainly in the discussions with the Panel, we spend a fair bit of time offering different explanations for what is happening.[62]

Thus broad money is used as an indicator of sorts by the mainstream members of the Panel. Like Congdon they do not use it in any mechanical way, but unlike Congdon, they are reluctant to draw any firm conclusions from broad money alone. Thus, when Gavyn Davies referred to broad money as a climate indicator, rather then a weather forecasting instrument, he meant that broad money movements give some idea of the general trend in the economy, but not in sufficient detail to be particularly useful - it is a bit like knowing that summer has begun but really wanting to know when is going to be the best time to book your holidays! More economically, Davies explains the role of broad money in his own theoretical framework as follows:

> *Davies* It impacts on my thinking about the climate because [the current low rate of M4 growth] is clearly telling you that there is not a substantial amount of willingness to borrow in the system at the moment. But it isn't a sufficient weight in my thinking to obliterate everything else, as it would be for Tim Congdon.[63]

The reasons that it does not 'obliterate' everything else are threefold. One is the traditional view of broad money growth which is that banks will lend whatever is asked of them. It is in this sense that the demand for broad money is held to reflect rather than cause activity. The second reason why broad money is less important for the rest of the Panel is that they believe that there are a variety of other ways in which companies can raise money which do not show up in M4 at all. Finally, the rest of the Panel question Congdon's belief that the government, through its policies, can actually control the quantity of money in any meaningful way.

Taking first the traditional view, that banks lend what is asked. If the banks are purely responsive, in that they grant whatever loans are required, then understanding the banking sector is not particularly important. What is important is understanding the factors behind demand and activity in the economy. Thus, as was shown in Chapter 1 the early National Institute model did not include broad money. In fact, it still does not. Andrew Britton explains why, using the example of the decline in credit at the recession which had ended:

> *Britton* You see if the decline in credit in the recession was simply due to firms deciding to cut back on investment because they didn't want to borrow any more, then it doesn't matter terribly if you're not monitoring credit very much.[64]

However, as was noted in Chapter 4, the so-called 'Credit Crunch' might actually have been caused in part by the behaviour of banks (e.g. attempting to reduce their liabilities and making borrowing tougher). If that is the case 'then if you ignore the bank's balance sheet position you are actually missing out one of the causative factors'. Britton describes the Institute's position on this issue as 'agnostic' but it is worth noting that:

> *Britton* We [the NI] have actually got a project going at the Institute now which is concerned with the role of the banking sector in the recent recession and the recovery. That is an element of behaviour that our model doesn't capture very well and we want to make sure we are not missing something fundamental ... when you get on to the late 80s, the credit base was expanded very rapidly and that was helping to stimulate expenditure, at least in the permissive sense of allowing expenditure to grow. It might even have been helping to push it up because there is no doubt that banks at that stage were very aggressively trying to sell credit. So that there may be an element of explanation which we are missing there, and perhaps the project we are just starting will help us to understand that better. [65]

Thus, there is a recognition, at least by the National Institute, that the behaviour of the banks may be important in determining the quantity of broad money. However, like just about everyone else, they are keen to distinguish their approach from that of Tim Congdon's:

> *Britton* It's a project in its own right and will actually involve going to talk to banks. That is the approach we are taking, rather than saying that this is the money supply and the money supply drives the price level as say Tim Congdon might.[66]

However, the very existence of the research project clearly acknowledges that there are now questions to be asked about the banks' role in the economy and that they may not be passive as may once have been the case. Thus it is recognised that:

> *Britton* Banks are an important part of the economy and some of the information you can get from looking at broad aggregates ... tells you about the behaviour of banks.[67]

In this way broad money is useful. However, it is only part of the story because there other ways for businesses to raise money and different ones may be favoured at different times. Because of this factor, the behaviour of banks and businesses is more difficult to analyse because there are now more options to consider. In particular, although the

current rate of broad money growth is low compared to what has been recorded in the past, it might not be particularly low in the context of the current economic situation. This point is not explicitly recognised by Tim Congdon, who appears to see no significant changes in the relationship between broad money and growth. He regards the recorded value of 3.9% as too low and has revised his growth forecasts down as a result. On the other hand, the rest of the Panel see in the M4 data the suggestion that the current situation is not just another instance of the general case. In other words, new and specific factors condition the interpretation of M4 at this particular time.

> *Britton* Just to answer your question about the current situation, it is as always very difficult to know what to make of M4. It slowed down very sharply during the recession and is still growing slowly but that's slow relative to the growth during the 1980s. I mean it is rising at a rate which may in fact now be consistent with a reasonable recovery. We know that disintermediation is now going on, you know the opposite of the transferring of business to banks, it is now being transferred back. Firms are now doing dealings off balance sheet.[68]

Thus as a result of some quite specific factors, both businesses and banks might be behaving in different ways to the past and thus the interpretation of broad money needs to reflect this. The importance of disintermediation is highlighted by both Andrew Britton (in the passage above) and also Patrick Minford:[69]

> *Minford* The issue is really one of how the wholesale market in money is working - are the banks being bypassed? The banks are having a very difficult time rebuilding their balance sheets, they have a lot of bad loans, and therefore have been very reluctant to lend. They have increased the threshold in terms of which they will lend, in terms of riskiness. They have been tougher with small businesses. All that sort of thing. Now what that has meant is that many businesses have gone direct to the wholesale markets, either borrowing on the wholesale market if they are big enough by issuing their own debentures, or else issuing equity. There have been a lot of rights issues, a lot of equity issues as the stock market has tended to improve and firms have been tapping it. So there seems to have been quite a lot of shifting away from the banks as the intermediary of finance.[70]

The conclusions that both Minford and Britton draw from this are surprisingly similar (for such different theoretical viewpoints). Both believe, unlike Congdon, that the low growth of broad money is not necessarily indicative of a problem:

Minford And so the M4 figures could really be distorted downwards I think. I think that they are telling a story, but again it may be an exaggerated story. If you look at the United States, you find the same sort of thing, very sluggish growth in M2, but the economy is growing faster than we expected and that's the result of this disintermediation.[71]

Britton It is quite possible that there is no real shortage of money and we'll find that the trend growth of M4 actually settles down into something in the range 0-5 [per cent].[72]

Summary and conclusions

From the above discussion we can see that the case of broad money is somewhat different to the case of narrow money. In the case of M0 it was the monetarist economist Patrick Minford who had to put in most of the intellectual work explaining the relationships between M0, technological change, interest rates and economic activity. For the rest of the Panel it was simply a 'fairly reliable' indicator. Minford on the other hand had to explain what made the present situation unique and different from the past such that 5% M0 growth in 1993 was consistent with sluggish growth and weak recovery, as opposed to the strong, even runaway, economic growth the same money supply figures would have heralded in the 1980s.

In the case of broad money however Tim Congdon sees the current situation as absolutely typical, with weak broad money supply growth meaning weak economic growth and a consequent downward revision of his forecasts for 1994. The majority of the Panel however feel it necessary to explain why there has been a change and the ways in which the low broad money figures do not represent cause for concern. Thus, the non-monetarists join Patrick Minford in highlighting the role of the banking sector in the economy. In particular, it is the behaviour of the banking sector which constitutes the atypical event which explains the difference between 1993 and the general case. Thus, the unusual lending spree of the banks in the 1980 has resulted in banks' balance sheets now being in unusually poor shape. As a result banks are unwilling to lend on the same terms as before and business is also keen to exploit new ways of raising finance. Consequently, although low by historical standards, it is quite possible that the recorded money supply figures are consistent with trend economic growth.

As Tim Congdon remarked:

Congdon Well, it is an interesting situation - we will see who is right and who is wrong...[73]

Economic Policy: Consensus and Effectiveness

This final section examines the policy recommendations made by the Panel and then compares them with the policies subsequently announced by the Chancellor. The focus is twofold. Firstly it examines the nature of the consensus on policy which the Panel appeared to reach despite their different theories, models and forecasts. It is suggested that the consensus is, at least in part, due to the relatively non-doctrinaire nature of the problem. However, a second and more sociologically important factor is the efforts of some the Panel members to be seen to achieve it.

The second part of the analysis focuses on the way in which the economically non-controversial reasons used by the Panel to support their recommendations are at odds with the political calculations the Chancellor must make - what makes sense as economics is not always good politics (as the previous Chancellor discovered!). In conclusion, it is suggested that one reason why the economists, like many other scientific advisers, are not particularly good at setting the policy agenda is that their analysis does not usually address the problems as they are perceived by the politicians.

Consensus

With the first Unified Budget due at the end of November (6 weeks after the publication of the October report), a major part of the Panel's Report focused on recommendations for economic policy. In February, when the Panel also focused on policy recommendations for the Budget, the extent to which they could sign up to a coherent and agreed set of economic policy recommendations was important for many commentators in assessing their worth. The same criteria were also important in October and, as we shall see, were consciously oriented to by several of the Panel members as they took part in the discussions and drafted the report.

The opening part of this section is concerned with the policy recommendations themselves and examines the extent to which the consensus can be said to be more than skin-deep. The latter part examines the social processes which lie behind the consensus. In particular, the analysis looks at the extent to which the consensus was *made* to happen by the Panel members themselves.

Unlike in Chapter 6, where the focus was on the medium term context for policy making and the different economic reasons for raising tax to reduce the PSBR, the focus in this Chapter is on the short term. In particular, given the range of forecasts for the medium term, and the quite genuine uncertainty about what would happen to the economy, what should the Chancellor do *now* in order keep as many options open as possible whilst, at the same time, neither damaging the recovery nor taking risks with the public finances.

Why the Chancellor Can't Leave the PSBR to Chance

The starting point for the Panel's policy recommendations is their diagnosis of the current situation and their agreement that 'the overriding imperative of policy is to ensure a sustainable fiscal position'[74]. However, as noted earlier in this Chapter (in the section which discussed the forecasts for demand), the Panel are by no means certain that economic growth is assured. Thus, there is no guarantee that growth will be sufficient to bring about an increase in tax revenues large enough to reduce the PSBR to an appropriate level. Because of this:

> [They] would therefore all support the announcement of tax or spending reforms, desirable in their own right, which would secure a net reduction in the PSBR. If it subsequently becomes clear that the PSBR will fall by more than is required, taxes can then be cut or public investment increased.[75]

The consensus view was thus that the 'public finances are probably just about on or within the border of sustainablity'[76] but that inaction now would be harder to correct later on. In other words, the consensus was *not* that some form of fiscal tightening was going to be needed. In fact, as the Report makes clear, 'there is no consensus whether further fiscal tightening will be necessary in practice'[77]. Rather the consensus is that the Panel:

> would all support further fiscal tightening over the next three to four years *if it became necessary* to ensure sustainablity[78]

When put like this, the consensus seems to be little more than a statement of the economically obvious - an unsustainable budget deficit is surely a bad thing in just about any school of economics and what economist could recommend an unsustainable policy? Consequently, it might be argued that a consensus on such an issue is relatively meaningless: 'How could they have thought otherwise and still made sense as economists' is the cynic's response.

In fact there is an element of this in the agreement:

Britton I think it is true that, just from the arithmetic of the budget and what is a sustainable deficit and what is not, [this] is a non-doctrinaire sort of issue. There was never a problem over that.[79]

However, this is not strictly true and part of the reason that there was not a problem was that Wynne Godley was unable to attend the meeting. As Britton explains this is not a personal comment on Godley, but rather that, because of his absence, certain (potentially controversial) issues were not discussed:

Britton I think if Wynne had been there, there might have been more debate as to whether this was a sensible question to ask. In an earlier stage in the discussion he was concerned to move away from the questions of long term budget stability and say "Well what difference is the fiscal position going to make to the level of output and therefore because it changes the level of output, what difference does it make in turn to taxes." In other words, to treat it as a simultaneous system. So I think we might have had more argument of that sort if he had been there. I think because we were all using models which we felt took account of this interaction anyway, there wasn't actually a great deal of disagreement as to the way in which the question of sustainability should be addressed, and that gave us a few paragraphs we could all sign up on straight away.[80]

However, to say that this is all there is to it is rather unfair and clearly overlooks what the Panel did achieve in their meetings. As Gavyn Davies pointed out, although there are some 'motherhood-and-apple-pie' statements in the report about the importance of fiscal sustainabililty, 'you would not have got most Keynesians to write that down ten years ago'[81] Thus, in just agreeing that the size of PSBR is a problem which cannot be ignored the Panel have come close to making economic history. Moreover, even if their analysis is not much more than an IS-LM story, in which increases in tax or reductions in public spending are offset by a relaxation of monetary policy, the issues addressed remain very important. As Gavyn Davies put it:

Davies There are big issues about what you want the overall stance of macro-economic policy to be. Do you want it to tighten or not to tighten. Usually you will not get economists to agree on that kind of thing ... I noticed in the public response to the last report, some people have said "You could only agree about the obvious". Well, that may be true. But I still think that there

> is a hell of a debate going on in Britain at the moment about whether the Chancellor should put up taxes in the Budget or not. This Panel has gone away and thought about it and come up with an answer. [82]

Certainly the aim in this section is not to repeat the ill-founded criticism that the Panel agreed in nothing of substance. Rather the aim is to explain the rationale behind the policy statement and to show how its production depended on the reasonableness of the Panel members and their own efforts to achieve it. In order to show just how much the Panel had to converge in order to produce the agreed statement, I will begin by outlining the range of views on offer. In other words, by outlining the 'gaps' which had to be closed, or at least papered over, if consensus was to be reached.

As noted above the recommendation put forward by the Panel was that, in the December Budget, a 'prudent' Chancellor would 'introduce a package of tax and spending reforms, desirable in themselves, which would reduce the PSBR over a number of years'[83] The Panel also went further than this and specified the sorts of reforms they would like to see and when they ought to be implemented. It is at this more detailed level that the consensus starts to break down. For example, on fiscal policy, Tim Congdon thought the tax increases should be implemented straightaway, whereas most of the others argued that, although the policies should be announced in 1993, the effect ought to be delayed until 95-96. On the other hand, Andrew Britton thought that if the taxes were not to take effect until 95-96, then decisions about them could safely be postponed until that time. Finally Minford thought that tax increases were completely unnecessary in terms of the PSBR, but did not object to them because they would create space for the kind of tax reform he favours. In terms of monetary policy, Sentance and Minford advocated easing monetary policy by different amounts, whilst the rest of the Panel favoured no change, or a relaxation only in the context of a deflationary Budget. The rest of this section outlines how, despite this range of views, theories and diagnosises, a single policy recommendation was nevertheless made to the Chancellor. It thus focuses more closely on the forecasts and analysis of the individual Panel members.

The majority of the Panel recommend that some sort of fiscal tightening be announced in the Budget. The most straightforward exposition of this view comes from Tim Congdon who states that 'there is no doubt that public sector borrowing is unsustainably high and that steps must be taken to reduce it'[84]. Thus, although tax increases have already been announced in the March Budget, it is Congdon's belief that

the 'structural Budget deficit probably remains at over 3 per cent of GDP (i.e. roughly £20 billion)'[85]. In terms of the necessary policy response, Congdon states that '3 years is surely quite long enough'[86] to reduce this to zero. Several of the other Panel members also take a similar view about the future size of the PSBR, but differ in that, unlike Congdon, they would prefer the measures to be 'phased in to avoid undue impact on [the] recovery during 1994'[87]. For example, David Currie writes that:

> *Currie* There is a need in the Budget to curb the PSBR, which we estimate at just under £50 billion this financial year ... we see the PSBR next year at £38 billion, still some 6 per cent of GDP ... At this level of borrowing, the interest paid on new borrowing cumulates rapidly, leaving a heavy burden of debt interest to be financed in the future.[88]

Apart from the high level of future interest payments, Currie also identifies further risks from such a policy:

> *Currie* High borrowing leaves interest rates more vulnerable to shifts in sentiment in financial markets, and may make it more difficult for the authorities to avoid an interest rate rise, particularly if the inflation background deteriorates.[89]

As a result, Currie thus argues for a shift in the balance of macroeconomic policy in order to place more of the restriction on fiscal policy, this enabling lower interest rates and hence a better exchange rate

> *Currie* Loose fiscal policy and tight monetary policy make it more difficult to maintain a competitive level of the pound without inflationary pressures emerging, impeding adjustment of the external current account deficit.[90]

Currie's view is thus more complex than that of Congdon who sees the question simply in terms of reducing the PSBR as quickly as practical. This difference stems from the different views of net trade - Congdon is optimistic about exports, whereas, as discussed in previous chapters, for Currie (and the rest of the mainstream economists) it remains a problem area. In terms of practical actions, Currie recommends 'Budget measures to curb borrowing amounting to some £3-4bn over and above [those] announced by the previous Chancellor'[91]. Like Congdon, Currie recognises that these measures could take the form of additional tax revenues or reduced spending and identifies several ways in which the reduction in borrowing could be achieved. For example:

Currie One option for the Chancellor is [to] take full advantage of the new integrated Budget to lower the public expenditure Control Total set by the Cabinet in July, to reflect lower inflation and the continued policy of tight control on public sector pay. This could cut public borrowing by some £2bn, requiring only modest additional action to raise revenues.[92]

However, Currie is concerned that such a policy (which Congdon doubts the government has the 'political will to implement'[93]) fails to address the long-run problems in the British economy (discussed in Chapter 6). Thus, he continues:

Currie A preferable alternative would be to maintain the Control Total and redeploy resources within the total from public consumption to public investment. This would allow support of those areas of public spending that help strengthen the longer run supply side performance of the UK economy, including education, training and R&D. In that case, he will need greater action on the revenue side. What should be avoided on the expenditure side is cuts in public investment to maintain public consumption.[94]

Andrew Sentance also provides a very similar analysis. Like Currie he believes 'that the medium term outlook for public borrowing is too high'[95]. Also like Currie he identifies the opportunity created by lower than expected inflation to reduce public expenditure:

Sentance Just as higher than expected inflation created overruns in spending in the late 1980s and early 1990s, it would seem appropriate to try and take advantage of a lower inflation climate to claw back some of these increases in the years ahead.[96]

Thus, unlike Currie, Sentance regards the option of reducing public expenditure in this way as a reasonable policy choice.

As noted in Chapter 6, Sentance was one of the economists who believes that the UK economy devotes too large a share of its output to consumption. It is not therefore surprising to find that, like Currie, he argues that 'a further fiscal tightening is [needed] to change the balance of fiscal and monetary policy in a way which is favourable to sustaining a higher level of investment and net exports.'[97]

This analysis is also important for Gavyn Davies who likewise argues that 'a further shift in the fiscal/monetary [mix] looks desirable in order to control consumption, boost investment and maintain a competitive real exchange rate'.[98] In

terms of practical action, Davies suggests (as he did in July) that the Chancellor should introduce 'consumption-reducing measures [which] should probably build up to at least 2-3 per cent of GDP over the next 4 years'.[99] However, unlike Currie Sentance and particularly Congdon, Davies believes that there is only a 'modest case' for these measures in terms of the sustainablity of the PSBR alone, writing:

> *Davies* On central economic forecasts, the PSBR should fall to 3.6 per cent of GDP by 1997/98. This compares with an appropriate target of about 2.5-3 per cent of GDP ... Given the immense uncertainty in all projections of this type, there is obviously no compelling case from a PSBR/financing point of view for further fiscal tightening.[100]

This aspect of Davies's argument is, in fact, supported by Andrew Britton who is one of the few Panel members who thinks that changes to fiscal policy should not be announced in the Budget. As noted above economic growth has been more or less consistent with the National Institute's forecasts (although inflation has been lower than expected) and, on the basis of these forecasts, Britton argues that

> *Britton* Our projections of the public sector financial position do not suggest the *need* for any further tax increases in the November Budget. The level of debt and borrowing implied for the medium term - in so far as this can be calculated with any degree of accuracy - does not appear imprudent.[101]

In interview, Britton argues against immediate action as follows:

> *Britton* Instead of going soon, you should wait because the recovery may stall and you already have a big tax increase coming along in the early months of next year. In the case of our particular forecast there is also a bit of anxiety about the effect of that on the price level. We could go over the top on the price level simply because of the indirect taxes [and] that would ruin your plans for reducing interest rates.[102]

However, in the Report itself Britton does not dissent from the view that it would be 'prudent' for the Chancellor to announce tax and spending reforms in the forthcoming Budget. The reasons he gives for this are as follows:

> *Britton* In the Panel meetings themselves, there was a lot of discussion about the relative risk. It wasn't so much a question of whether you have to, but whether perhaps you nevertheless ought to ... Should you, in some sense, try and play safe - would you feel safer if you had a bit of tax increase under your belt and you could always give it away again? Well I certainly accept

> that argument ... I wouldn't mind so much if he's pencilled in something for later years, because I regard this pencilling in as a rather abstract exercise which can always be undone at the next budget if you don't like it ... [But] if you have got firm plans - you know that you want to widen the VAT base as soon as possible or you want to abolish mortgage interest relief - there is no great harm in announcing those plans. It might even reassure the markets that you have got the situation under control.[103]

Thus, even though announcing tax and spending plans in advance is not something that Britton would personally recommend, he is prepared to concede that this could still be a reasonable strategy for the Chancellor to follow. Finally, Patrick Minford's contribution to the debate is also very interesting because his forecast for economic growth is very different. As noted in previous chapters, Minford believes that, if the PSBR is almost entirely cyclical, then:

> *Minford* first the public borrowing we see would melt away [and] secondly there would be no need for the public spending programmes allegedly needed to boost competitiveness. Thirdly, interest rates, currently at around 4 per cent adjusted for inflation, could come down without risk in order to underpin the still fragile recovery[104]

Minford then goes further and disputes the claim that 'even if that were all true, keeping policy as it is would not matter because the economy would in time recover anyway [and] taxes could then be cut'.[105] In response to this claim, Minford argues that:

> *Minford* reversing damaging tax and spending decisions is not easy, because they create vested interests in the continuation; spending ministries always find good ways of spending tax revenues and spending programmes acquire lives of their own. Even if reversal could be guaranteed there is still the loss of output meanwhile, as stagnation is unnecessarily prolonged.[106]

Thus, to sum up, behind the consensus that a prudent Chancellor 'would introduce a package of tax and spending reforms' to ensure control of the PSBR we have the following views:

- Tim Congdon arguing that the PSBR is unsustainable and that fiscal reform should start straight away
- David Currie and Andrew Sentance arguing that the PSBR is unsustainable and that measures to correct this need to be phased in.

- Gavyn Davies arguing that the PSBR is probably OK, but that the balance of policy needs to shift to boost exports (an argument also supported by Currie and Sentance)
- Andrew Britton arguing that no policy announcement is needed at the moment because the PSBR is probably already under control
- Patrick Minford arguing that the PSBR was not the problem - it was ensuring the recovery which mattered.

The question which I want to address in the final paragraphs of this section is how did the Panel get from this disparate starting point to a single policy recommendation. There is no reason why this had to happen, and at least one of the Panel disputes the suggestion that they should try to reach consensus recommendations:

> *Congdon* I don't think the purpose of the Panel is to be consensual, and that certainly isn't what we were told. That certainly isn't my understanding of its purpose. It is to bring together these individuals so that they can express their views. It may happen that on some things they agree, but it isn't the purpose to reach consensus ... there is nothing of that kind at all. I mean it may be that other Panellists think that there is [but] nothing of that kind has been conveyed to me as part of the purpose of the Panel. I certainly don't see the Panel in those terms, and if I don't agree with what the rest of the Panel is saying I will indicate that I don't agree with it ... but there can hardly be any disagreement with the point that in the end, the growth of the public debt must be brought under control.[107]

Thus, for some, as noted at the beginning of this section, the reason for the consensus in the non-doctrinaire nature of the problem. Patrick Minford also remarked:

> *Minford* On this occasion, the disagreements between the Panel about what exactly the policy would be were not very great in fact ... I think most people assumed that they would cut interest rates in response to a fairly weak economy, and most people also assumed that they would take some steps to tighten fiscal policy one way or another.[108]

However, this remark (which focuses more on the assumptions than the recommendations) downplays the significance of Minford's own reasonableness in the meetings. As one of the outliers, including Minford's stance (which was the 'most different'[109]) was crucial to producing the consensus. For example, Andrew Britton

commented, reaching agreement was not simply because the Panel had started off with very similar views:

> *Britton* Patrick ... who had the very optimistic projection of the borrowing requirement, was quite prepared to concede that there was a serious risk and that the whole question of sustainability ought to be discussed. Even though, in his projection, it was a problem that disappeared almost immediately he was quite prepared to use other people's forecasts as a basis for discussion.[110]

Similarly, Andrew Sentance noted that Minford's reasons for endorsing the policy recommendation were rather different reasons to the rest of the Panel:

> *Sentance* Patrick believes that, later in this Parliament, the Chancellor, assuming he follows our advice, will have a lot of scope for reducing taxes in the way that Patrick would like to see. So he is relaxed about it. He doesn't see this as being necessary for fiscal tightening means - he sees that other people want it for fiscal tightening means, but that's not why he's wanting it, although he has agreed to put his name to it.[111]

Congdon may not feel these pressures but the majority of the Panel do. For example, in a previous interview Minford commented that one of the 'interesting things' about the Panel's first ever meeting was that:

> *Minford* we were all trying to be nice I think, apart from Tim who was all at arms length, [and] to communicate as economists, as part of this [scientific] community
>
> *Evans* You felt you had to make an effort?
>
> *Minford* Yes, make an effort [for] our community of economists, and the community at large ... there was that sort of spirit about it, which was quite interesting.[112]

Minford's view is that if the group is to survive and become truly useful it must develop its own 'group dynamic where people are willing to actually confront evidence and modify their positions over time ... Otherwise it is simply going to be a bunch of people who disagree the whole time'.[113] As a result of this, Minford believes that:

> *Minford* the only way it will be successful is if it develops a group ethos. At the moment it hasn't ... but in a way that doesn't matter because it can still develop over time ... I think that we have all got different views but, at the

end of the day, there are important public interest issues to be settled here and we can't afford to let our vanity or whatever it is get in the way of a sensible discussion.[114]

Certainly Minford's own efforts in this direction have not gone unnoticed by his colleagues with Andrew Britton noting that 'he is certainly being very reasonable himself - which is not the reputation he once had'[115]. Interestingly, by October, Gavyn Davies can see that progress is being made in developing this group ethos:

Davies I do think the Panel is showing some signs of developing in ways which are separate from simply the three meetings per annum cycle. I think we are going to have additional meetings about different subjects, more subject related than forecast related. So those three meetings will exist but I think in addition there will be other things, which I would think is going to be good ... I still think we are kind of groping our way towards a role, but I think that the last set of meetings were actually pretty good. I think they were good meetings in themselves and I think there was more of a corporate spirit ... people are getting to know each other, and feeling part of the same operation as opposed to just visitors to a meeting. I think more of us are buying into the idea that we want to make this thing work.[116]

To conclude we can see that there were several different views on offer at the October meeting and there was no obvious way how they reduce to a single policy. Nevertheless there was also a feeling, at least amongst some, that if the Panel was going to be judged useful, then it was important that they at least minimised their differences and agree a single strategy for the forthcoming Budget. That they did is not entirely attributable to the nature of the problem. As we saw, they came to the meeting with a range of different views. Thus, the consensus is also due, in part, to the Panel's efforts to make it so.

Several of the mainstream members actually made this point quite explicitly in interview, although each stressed slightly different reasons for wanting consensus. For example, Andrew Britton highlighted the role of press and public opinion, saying:

Britton I think you are right about seeking consensus. We felt, particularly after the July Report, which got rather a bad press, that people found the arguing in public a bit tedious and that they would prefer to emphasise the agreements rather than the disagreements.[117]

On the other hand, Andrew Sentance agreed that there was an effort towards reaching agreement, but 'not to placate the media'[118]. Rather:

> *Sentance* there was a feeling that here was a man we were supposed to be reporting to - the Chancellor of the Exchequer - and he was going to have to take some tough decisions in the Budget. He was going to look at this report and he was going to say 'What is this Report telling me about what I should do in the Budget?' And if we couldn't come to some sort of strategy that really came out of the views of the Panel then I think, quite legitimately, he would say 'This is all very well, but it is not helping me a great deal'. [So] it was an attempt by us to do our job - which is to advise the Chancellor.[119]

The effort towards convergence was also noted by Gavyn Davies, who said:

> *Davies* I think there was more agreement coming out of that meeting than there was going into it, which suggested that there was a willingness to compromise in order to reach a common objective ... I do think there was a genuine tendency for people to see each other's point of view and amend their own position as a result of the discussion. I don't want to overstate this. I don't think there was that much disagreement going in, and I don't think there was 100 per cent agreement coming out, but I think there was a tendency to move towards agreement, which I don't think was there in the previous two meetings.[120]

The important point to draw from this is that the Panel actively worked to bring about consensus and hence closure of the debate. The reasons for this, in various ways, related to the perception of others - the media, the Chancellor, other economists and so on. As Gavyn Davies pointed out:

> *Davies* I think that the Treasury has given us a massive opportunity, both as individuals and as representatives of economics outside the Treasury.[121]

It seems that this responsibility is one taken seriously by the Panel and one of the key factors in explaining why, in October, they were able to find ways of working around their differences and speaking to the Chancellor (and the public) with a single voice. The next section examines the effectiveness of this 'single voice' on the Chancellor's ear.

Effectiveness

This, the final section of this chapter, briefly examines the effectiveness of the Panel in shaping economic policy. The measure of effectiveness used is simply the extent to which the Chancellor can be seen to heed the advice given to him by his advisors. The previous section outlined in some detail the policy recommendation the Panel made to the Chancellor. This section thus begins by briefly outlining the policies adopted by the Chancellor.

Chancellor Kenneth Clarke's maiden Budget, which was preceded the week before by a 0.5 per cent cut in interest rates, was widely hailed as a success by both his fellow Conservative MP's and by City economists. The Chancellor began his Budget by saying that:

> My first priority has been to sustain the economic recovery now underway and to create the right climate for growth and jobs. I have been determined to take no risks with inflation. To achieve these objectives, the task of my first Budget has been to set the government's finances on a sustainable path for the rest of the decade.[122]

Before looking at the detail of the Budget, it is obvious that the Panel appear to have been successful in setting the policy agenda. The similarity between the task the Chancellor set himself and the Panel's advice 'that the overriding imperative of policy is to ensure a sustainable fiscal position'[123] is clear enough. The suggestion that the Chancellor did follow the Panel's advice is also reflected in the warm reception they gave the Budget. For example, *The Independent* reports the Panel as having 'applauded [it as] "brave", "skilful" and "an appropriate start."'[124]

However, it would not be fair to say that the Budget wholly reflected the Panel's priorities, although it clearly did in some respects. For example, direct tax rates were not raised. However, the levels of most allowances were frozen and Mortgage Interest Relief was reduced to 15 per cent. In addition the tax base was widened to include new taxes on air travel and insurance. These tax raising measures were however offset by increased expenditures and, of the £5.5bn reduction in the PSBR announced (far bigger than anything the Panel considered necessary or even possible) only £1.75bn of this saving came from the changes to the tax and benefit system. The vast majority of the saving (about £3.5bn) came from a reduction in the 'contingency reserve' which was halved from £7bn to £3.5bn. Nevertheless, a Budget which reduced the PSBR was

definitely what the Panel wanted and so to that extent they can be said to have been influential.

However, when the Budget measures are looked at in more detail it is apparent that several issues identified as important by a number of the Panel are not addressed in the Budget. For example, in their Report the Panel listed as 'an important caveat' that they would 'caution against anything more that a modest further tightening of the fiscal position for 1994-95 because of the large tax rises already in place'. Clearly the Chancellor did not accept this concern and, as a result, several of the Panel were concerned about the effect of the Budget on the recovery - fearing that it may slow growth. For example, Andrew Britton was concerned about the effect of the Budget on growth saying that:

> *Britton* By raising taxes, he has reduced the amount people can afford to spend ... The measures he has announced could clearly cause hardship unless the economy expands more rapidly that we are forecast.[125]

As a result of this deflationary impact some of the Panel reasserted their recommendation that 'any further tightening of fiscal policy should be offset with lower interest rates'[126]. Gavyn Davies was relatively sanguine and felt that the Budget could justify a further 1 per cent cut in interest rates over the following 6 months. Wynne Godley on the other hand, whilst accepting that the Chancellor was right to take a tough line in the Budget, was more concerned, warning that:

> *Godley* I do think that it will cause a very severe check to the expansion, which in any case is not very robust ... Interest rates ought to be cut again.[127]

Finally, Patrick Minford felt that interest rates could be lowered as a result of the Budget, although for different reasons. He had no worries about the growth rate and felt that, by ensuring that the PSBR was firmly under control the last 'fiscal excuse' for not cutting interest rates was now removed.[128]

Thus, we can see that although the Panel were influential in terms of the basic Budget judgement (i.e. ensure fiscal sustainablity) they were not so influential on monetary policy. There were also other ways in which the Panel's priorities were not fully reflected in the Chancellor's speech. One thing that the Panel were almost unanimous on was that the PSBR should be reduced through increases in taxation. However, in the Budget most of the reduction in the PSBR came about through reductions in public expenditure.

Although Tim Congdon's recommendation for tax increases was based on the assumption that there was not the political will to implement public spending cuts on a sufficient scale, other members of the Panel had argued that increases in expenditure were needed in order to improve economic performance, in particular with regard to net exports and the Balance of Trade. For example, Gavyn Davies recommended that taxes be increases and half the extra revenue be used to 'boost public investment, education and training.'[129] A similar economic priority is to be found in David Currie's submission. In addition, whilst correctly anticipating that the lower than expected inflation *could* be used by the Chancellor to reduce public expenditure, Currie actually counselled against this, saying:

> *Currie* A preferable alternative would be to maintain the Control Total and redeploy resources within the total from public consumption to public investment. This would allow support of those areas of public spending that help strengthen the long run supply side performance of the UK economy, including education, training and R&D.[130]

A similar concern was also expressed by Wynne Godley who wrote that:

> *Godley* neither tax increases or public expenditure cuts are needed at the moment and that to implement them immediately would be unnecessarily destructive. Indeed there remains a case ... for actually increasing public investment.[131]

There was therefore within the Panel, as noted in Chapter 6 which discussed the 'twin deficit problem' in more detail, a series of arguments made by the Panel members that fiscal reform was needed for two reasons. One was that the PSBR was just too big to be reduced by economic growth alone. This set of arguments which was backed by Congdon, Currie, Davies and Sentance was more or less adopted by the Chancellor.

On the other hand, there was a second set of arguments (backed by Britton, Currie, Godley, Davies and Sentance), which centred around the need to improve the Supply-Side of the economy, reduce consumption and increase investment and exports which did not make it through to the political agenda. The rest of this section suggests some reason why this might be.

One important reason why the Panel were influential in setting the 'Budget judgement' was that they spoke with one voice. As noted above, some of the Panel felt that they made a deliberate effort to do this, others that it just happened due to the nature

of the question. Nevertheless, it remains the case that, as in March, the Panel are more effective when they are seen to produce some sort of closure. The contrast between the case of the PSBR and the trade deficit is thus one between a consensus and a contest - the sustainability of the PSBR is (made to be) a consensually agreed problem, the trade deficit has not reached that point yet.

However, it has to be noted that, despite the closure achieved by the Panel on the issue of the PSBR the Chancellor deviated from their recommendations is several important ways. In particular, he did impose a significant tightening of fiscal policy in the first year, he did this by reducing expenditure rather than raising taxes and he did not offset the deflationary impact of the fiscal measure with monetary policy. Why then did the Chancellor, after accepting the basic premise of the Report, ignore so much of the detail. I would argue that part of the reason was that the majority of the Panel's arguments were concerned solely with the economic reasons for a particular policy. This is of course how it should be - they are professional economists and it is in this capacity that they advise the Chancellor. However, the Chancellor is lobbied by many different interest groups. This is of course recognised by the Panel who frequently observe that their assumptions about what the Chancellor will do differ from their recommendations as to what he should do.

In the case of the Budget Deficit, the Panel present an economic argument for (a) reducing the Deficit and (b) not doing it straightaway. The economic balancing of risks they performed is summarised in the Table overleaf. However, it is clear that the political cycle does not figure predominantly in the Panel's thinking although it is implicit in the final recommendation which is that:

> [The Panel] would therefore all support the announcement of tax or spending reforms, desirable in their own right, which would secure a net reduction in the PSBR. If it subsequently becomes clear that the PSBR will fall by more than is required, taxes can then be cut of public investment increased.[132]

The implicit political lollipop here is that if the taxes are announced now then there is a chance that the Chancellor will be able to assist his party's electoral chances later in the Parliament by undoing them in a tax cutting pre-election Budget designed to win votes. However, it is apparent from the Budget that this is not enough to sway the Chancellor who bases his Budget as much on political calculation as he does economic calculation.

For example, an important part of the December Budget was the decision to increase the benefit payments to compensate pensioners affected by the imposition of VAT on fuel by his predecessor. Indeed it is instructive to compare the two Budgets in this regard.

Figure 1: Summary of Arguments For and Against Further Fiscal Tightening

For	Against
A position which does no more than ensure sustainability may lead to an inappropriately high debt stock.	There is already a considerable fiscal tightening in place for next year.
If a further tightening of policy may be required at some time, it is better on economic grounds to act sooner rather than later and taxes can always be cut later.	The recovery remains relatively fragile. Although the effect of the increases and / or spending cuts on the economy could be offset by lower interest rates, it would be impossible to 'fine tune' the result.
The slow pace at which the PSBR is projected to fall over the medium term in the absence of further measures could limit the room for manoeuvre - for example, if faced with an economic downturn in later years.	Tax increases or cuts in public investment might damage the supply performance of the economy.
A tighter fiscal stance would facilitate lower interest rates which would help to maintain a competitive exchange rate.	The cost of waiting a year or so to take any action might not be great.

Norman Lamont's March Budget, although economically so successful that Kenneth Clarke's December one amounts to little more than a 'fine-tuning the Lamont approach'[133], was politically disastrous. Clarke's Budget, on the other hand, which 'did nothing for the economy'[134] apart from reassure it, is hailed as a great success. The point to make here is not that economists should become politicians. Rather it is that problems which the Panel diagnosed as important for the future of large numbers of their fellow citizens were simply not addressed in the Budget. Consequently the structural problems they identified still remain, potentially holding back economic growth in the UK and prolonging the waste of human lives.

The conclusion I draw from this is that if the Panel want to make their expertise and professionalism count, they clearly need to find ways of communicating it which make sense to their putative audience - in this case the politicians. Of course this does not mean sacrificing their science - after all it is their science which has allowed them to

make the diagnosis in the first place. The problem really is more akin to doctor trying to persuade patients to give up smoking. There is no doubt that doctors believe smoking to be dangerous as a result of scientific medical research which they find credible. Thus, the first step is always achieving closure. However, even after this, the patients still have to be persuaded to change their ways - the science is thus only the beginning of the process, converting it to action is equally important. It may be argued that this is not science, but if scientists cannot make the case for their own science then who can? The alternative, as demonstrated by the mainstream members of the Panel, is at best only a minor influence and at worst irrelevance.

Notes

[1] Prior to 1993, spending plans had been announced in the Autumn Statement and tax plans in the March Budget.

[2] *Report of Panel of Independent Forecasters, October 1993*, para 1.

[3] In other words, the negative output gap was still increasing, albeit at a slower rate. thus, in terms of the metaphor of progress often used for economic growth, the economy was still losing ground or going backwards.

[4] David Currie, *Submission to Report of Panel of Independent Forecasters, October 1993*, para 15.

[5] Source: David Currie, op cit. note 4, para 14.

[6] Tim Congdon, *Submission to Report of Panel of Independent Forecasters, October 1993*, para 6.

[7] Andrew Britton, *Submission to Report of Panel of Independent Forecasters, October 1993*, paras 9-11.

[8] Tim Congdon, op cit. note 7, para 6.

[9] David Currie, op cit. note 4, para 1.

[10] Andrew Sentance, Submission to Report of Panel of Independent Forecasters, October 1993, para 1.

[11] see e.g. quote from David Currie in Chapter 5: '[This] is the characteristic response of the British economy'.

[12] Andrew Britton, op cit. note 7, para 14.

[13] David Currie, op cit. note 4, para 2.

[14] Gavyn Davies, *Submission to Report of Panel of Independent Forecasters, October 1993*, para 17.

[15] See Chapter 5

[16] David Currie, op cit. note 4, para 2.

[17] Tim Congdon, op cit. note 6, para 3.

[18] Tim Congdon, op cit. note 6, para 3.

[19] Patrick Minford, *Submission to Report of the Panel of Independent Forecasters, October 1993*, para. 12.

[20] Patrick Minford, op cit. note 19, para. 1.

[21] Patrick Minford, op cit. note 19, para 10.

[22] David Currie, op cit. note 4, para 3.

[23] Patrick Minford, op cit. note 19, para 5.

[24] Patrick Minford, interview 25 October 1993, pp. 14-15.

[25] Patrick Minford, op cit. note 24, p. 15

[26] Gavyn Davies, interview 27 October 1993, p. 18

[27] Andrew Sentance, interview 29 October, p. 17.

[28] Andrew Sentance, op cit. note 27, p. 18.

[29] op cit. note 2, para 19.

[30] op cit. note 2, para 19.

[31] Gavyn Davies op cit. note 14, para 14

[32] Andrew Britton, op cit. note 7, para 9.

[33] Wynne Godley, *Submission to Report of the Panel of Independent Forecasters, October 1993*, para. 1.

[34] Wynne Godley, op cit. note 33, para. 4

[35] Patrick Minford, op cit. note 19, para 12.

[36] For an early STS style analysis of the controversy surrounding monetary economics see: Arouh, A. (1978) 'Empirical Testing and Theory Validation: A Structural Explanation of Persistent Controversy in Economics.' PhD thesis, University of Edinburgh. I am grateful to David Edge and an anonymous *Social Studies of Science* referee for alerting me to this work.

[37] There are several definitions of the money supply which vary according to the range of bank deposits which they include. The most restrictive or narrow definition is M0 which includes only money which exists as notes and coin, it does not include bank balances and deposit accounts. The other definition of money commonly used is M4 which includes M0 plus moneys held in savings, deposit and loan accounts - a much broader definition of what is to count as money.

[38] Andrew Britton, interview 27 October 1993, p. 2.

[39] This may however be a function of Congdon's own desire to differentiate his 'product' from that of other economic forecasters.

[40] Tim Congdon, interview 29 October 1993, p. 9.

[41] Andrew Britton, op cit. note 38, p. 2.

[42] Andrew Britton, op cit. note 38, p. 2.

[43] Gavyn Davies, op cit. note 26, p. 2.

[44] Tim Congdon, op cit. note 40, p. 9.

[45] Andrew Sentance, op cit. note 27, p. 1.

[46] Andrew Britton, op cit. note 38, p. 2.

[47] Andrew Sentance, op cit. note 27, p. 1.

[48] Patrick Minford, op cit. note 24, p. 1.

[49] As Professor Sheila Dow has pointed out, this formulation assumes the money supply is perfectly endogenous.

[50] Patrick Minford, op cit. note 24, p. 2.

[51] Patrick Minford, op cit. note 24, p. 2.

[52] Patrick Minford, op cit. note 24, pp. 2-3.

[53] Patrick Minford, op cit. note 24, p. 5.

[54] Patrick Minford, op cit. note 24, p. 4.

[55] Gavyn Davies, op cit. note 26, p. 2.

[56] Tim Congdon, op cit. note 6, para 4(ii).

[57] Gavyn Davies, op cit. note 26, p. 2.

[58] Tim Congdon, op cit. note 40, p. 7.

[59] Tim Congdon, op cit. note 6, para 4(ii).

[60] Tim Congdon, op cit. note 40, pp. 7-8.

[61] Tim Congdon, op cit. note 40, p. 8.

[62] Andrew Britton, op cit. note 38, pp. 2-3

[63] Gavyn Davies, op cit. note 26, p. 2.

[64] Andrew Britton, op cit. note 38, p. 3.

[65] Andrew Britton, op cit. note 38, p. 3.

[66] Andrew Britton, op cit. note 38, p. 3.

[67] Andrew Britton, op cit. note 38, p. 3.

[68] Andrew Britton, op cit. note 38, pp. 3-4

[69] It was also referred to by David Currie in Chapter 4

[70] Patrick Minford, op cit. note 24, p. 3.

[71] Patrick Minford, op cit. note 24, p. 3.

[72] Andrew Britton, op cit. note 38, p. 4

[73] Congdon, op cit. note 40, p. 10.

[74] op cit. note 2, para 36.

[75] op cit. note 2, para 38.

[76] op cit. note 2, paras 34 & 37.

[77] op cit. note 2, paras 36.

[78] op cit. note 2, paras 36, emphasis added.

[79] Andrew Britton, op cit. note 38, p. 8.

[80] Andrew Britton, op cit. note 38, pp. 8-9.

[81] Gavyn Davies, op cit. note 26, p. 15

[82] Gavyn Davies, op cit. note 26, p 16-17

[83] op cit. note 2, Introduction.

[84] Tim Congdon, op cit. note 6, para 15.

[85] Tim Congdon, op cit. note 6, para 16.

[86] Tim Congdon, op cit. note 6, para 16.

[87] David Currie, op cit. note 4, para 10.

[88] David Currie, op cit. note 4, para 8

[89] David Currie, op cit. note 4, para 8

[90] David Currie, op cit. note 4, para 8

[91] David Currie, op cit. note 4, para 10.

[92] David Currie, op cit. note 4, para 10

[93] Tim Congdon, op cit. note 6, para 15.

[94] David Currie, op cit. note 4, para 10

[95] Andrew Sentance, op cit. note 10, para 13.

[96] Andrew Sentance, op cit. note 10, para 12.

[97] Andrew Sentance, op cit. note 10, para 14.

[98] Gavyn Davies, op cit. note 14, para 4.

[99] Gavyn Davies, op cit. note 14, para 35.

[100] Gavyn Davies, op cit. note 14, para 4.

[101] Andrew Britton, op cit. note 7, para 29

[102] Andrew Britton, op cit. note 38, p. 6.

[103] Andrew Britton, op cit. note 38, pp. 6-7.

[104] Patrick Minford, op cit. note 19, para 17.

[105] Patrick Minford, op cit. note 19, para 18.

[106] Patrick Minford, op cit. note 19, para 18.

[107] Tim Congdon, op cit. note 40, pp. 13-14.

[108] Patrick Minford, op cit. note 24, p. 9.

[109] Andrew Britton, op cit. note 38, p. 6.

[110] Andrew Britton, op cit. note 38, p. 9.

[111] Andrew Sentance, op cit. note 27, p 14.

[112] Patrick Minford, interview 31 March 1993, p. 20.

[113] Patrick Minford, interview July 1993, p. 2.

[114] Patrick Minford, interview July 1993, pp. 2 and 9.

[115] Andrew Britton, op cit. note 38, p. 8.

[116] Gavyn Davies, op cit. note 26. pp 12-13.

[117] Andrew Britton, op cit. note 38, p. 8.

[118] Andrew Sentance, op cit. note 27, p. 13.

[119] Andrew Sentance, op cit. note 38, p. 12.

[120] Gavyn Davies, op cit. note 26, p 16.

[121] Gavyn Davies, op cit. note 26, p 13.

[122] Source: Budget Speech as reported in *The Times*, 1 December 1993.

[123] op cit. note 2, para. 36.

[124] Lisa Vaughan, 'The Budget: "Wise Men" advise interest rate cut: Economic advisers' verdicts' *The Independent*, 1 December 1993, Budget Page 12.

[125] Andrew Britton, quoted in Vaughan, op cit. note 124.

[126] op cit. note 2, para 42.

[127] Wynne Godley, quoted in Vaughan, op cit. note 124.

[128] Patrick Minford, quoted in Vaughan, op cit. note 124.

[129] David Currie, op cit. note 14, para 5.

[130] David Currie, op cit. note 4, para 10.

[131] Wynne Godley, op cit. note 33, para 10.

[132] op cit. note 2, para 38.

[133] Andrew Marr, 'The Budget: Another Macleod on the horizon?', *Independent*, 1 December 1993, Budget p. 13.

[134] Pennington, 'Pleasing Gamble Might Come Off', *The Times*, 1 December 1993, p. 29.

Chapter 8

This, the final chapter, summarises the analysis of the previous chapters and draws some conclusions about its implications. It is divided into three main sections. The first section, recounts the forecasts made during 1993 and highlights how the economists integrated the particular features of the moment with the more general stories they wanted to tell. In so doing I set out more clearly the theoretical framework which I have developed during this thesis - that of the tension between the 'general' and the 'particular'. The discussion thus continues one of the key questions of science studies: 'What does it mean for two things to be the same?'

The second section continues this theme but focuses on how the Panel of Forecasters understand the events of 1994 when they too look at them as 'outsiders'. By focusing on their own retrospective analyses of 1993, I once again highlight the way in which particular instances are made into examples of a more general case. This section also draws on the uncertainty of econometrics and the importance of expertise in economic modelling. Finally, the importance of consensus building in communicating science is discussed.

The third and final section of this chapter switches the focus from economics and economic modelling and back to the sociology of scientific knowledge. It thus returns to the issues of symmetry and neutrality raised in Chapter 1 and reappraises the discussion in the light of the research presented. It is argued that the sociological study of social science raises special problems in that the sociologist's own expertise often conflicts with that of the other social scientists. In conclusion, it is argued that the research strategy and methodology employed in this thesis offers a workable solution and, potentially, opens up new fields of inquiry to science studies

February 1994 Report

This chapter begins by examining the Report of the Panel of Independent Forecasters published in February 1994. Although much of the Report deals with what lies ahead in 1994 and beyond, the Report also contains a retrospective analysis of the events of 1993 and it is this aspect of the Report which is the topic of this section. In particular, I compare the Panel's forecasts with the actual outcomes and examine what they learnt from this. The focus is thus on the ways in which the Panel themselves interpret the data and judge their own performance. As we shall see, they all would admit that they

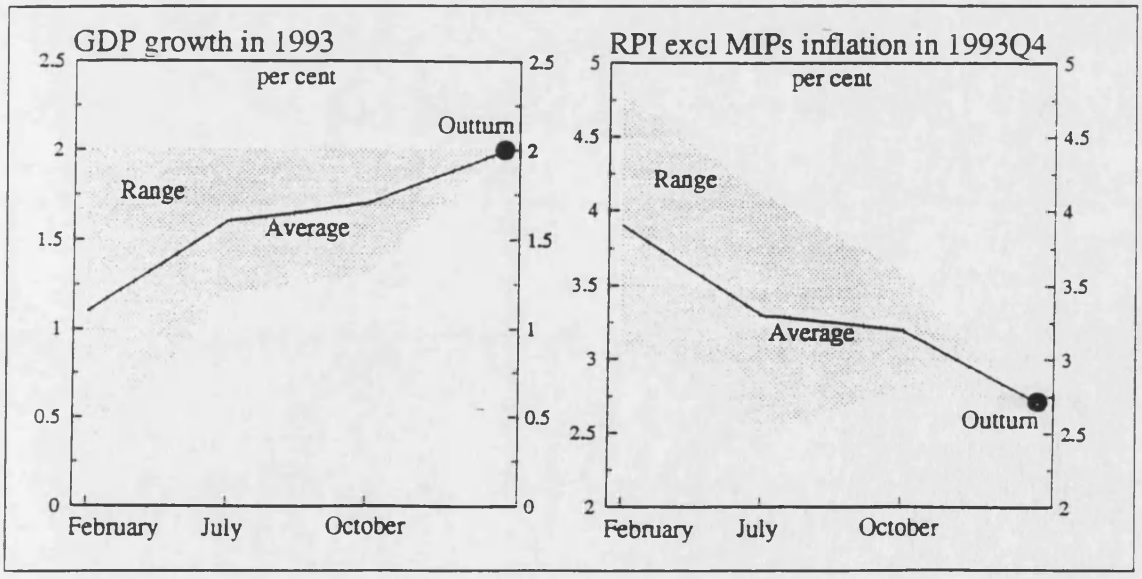
have made mistakes and misjudgements. However, it is also true that they do not see anything in the events of 1993 which forces them to fundamentally rethink their underlying theoretical ideas.

When compared with the forecasts made in February 1993 (i.e. a year earlier) it is clear that the Panel as a whole under-predicted economic growth whilst simultaneously over-predicting inflation:

> Preliminary figures show that GDP rose by 2 per cent in 1993 ... The main contributions to growth in 1993 came from personal consumption and exports, the latter partly offset by import growth to give a small positive contribution from net trade. Investment has shown little sign of picking up so far. Underlying inflation fell slowly through 1993, ending the year at 2.7 per cent. The unemployment count peaked in January 1993 and then fell by over 200,000 through the rest of the year. The trade picture has been obscured by the move to the Intrastat system for EC trade data, but overall the current account deficit appears to have been about the same as in 1992.[1]

This is feature of the forecasting is clearly illustrated in the chart below:

Figure 1: Forecasts and Outturns for GDP Growth and Inflation in 1993[2]



As can be seen, the Panel's forecasts converge on GDP from below and inflation from above (graphically illustrating the analysis in Chapters 6 and 7, where the tendency for the Panel to reduce forecasts for inflation whilst simultaneously increasing forecasts for growth was noted). There it was argued that this implied that the inflation-output trade-

off was better in this particular post-recession period than previous experiences had suggested was likely. This point is also acknowledged by the Panel whose interpretation of this better than expected outcome was as follows:

> *Currie* Looking back at the forecasts for 1993 that we made over the course of the year, we note that out forecasts of growth converged on the outturn almost entirely from below, while our forecasts of inflation converged almost entirely from above. This points to a better short-term trade-off between inflation and growth than we were expecting a year ago. This is supported by other developments: a better trade balance than most of us expected, as far as we can tell, and sharp falls in unemployment which none of us expected.[3]

Thus we can see that the Panel recognise that the inflation-output trade-off had turned out to be better than their econometric models had suggested. In addition, they also identified two other factors which make 1993 different from more typical post-recession periods - one is that net trade had improved by more than most of the Panel expected, the other is that unemployment had fallen by more than anyone had expected. The question now is whether these historically-unusual events are the beginning of a new orthodoxy (i.e. the inauguration of a new economic pattern) or merely a particularly unusual instance of the previous patterns. The Panel overwhelmingly favour the later interpretation and thus stick with the ideas, stories and models they used for 1993 when forecasting 1994 and beyond. In fact, they even suggest that the outcome for 1993 was not so unusual after all:

> The events of [1993] do not cause any of us to change our (differing) views of the UK economy fundamentally, and in the main have not led us to make substantial changes to our forecasts for 1994 and beyond. The happy combination of steady growth and falling inflation is not unprecedented in the early stages of recovery.[4]

In the rest of this section I examine the responses of the individual forecasters in more detail before, in the next section, examining some of the issues raised by them.

The 'Devaluationists'

In Chapter 5, it was argued that the Panel's forecasts fell into two main types - there were those who stressed the importance of the devaluation in forecasting the economy and those who relied on equilibrium concepts such as the natural rate of unemployment. The basis of the 'Devaluationist' account was that:

Britton Without the stimulus from the devaluation of sterling and the cuts in interest rates, output would hardly have recovered at all last year [1993], and unemployment would have risen well above 3 million.[5]

For the Devaluationists, the problems which they had to account for in 1994 were that, the devaluation notwithstanding, growth in 1993 had been higher and, more importantly, inflation lower than they had expected. In addition, unemployment had fallen sooner and more rapidly than in previous economic cycles and the net trade figures were surprisingly good. In short, the pessimists were just too pessimistic about the future. This fact is readily conceded by the most pessimistic pessimist of all, Wynne Godley:

Godley The short term forecasts which I have provided for the Panel since its inception have all been too pessimistic. Output has risen rather more than I expected, the deficit in the balance of payments and inflation have both been lower; and unemployment has apparently fallen, although it is still possible that this may be largely a statistical illusion.[6]

What is important in the context of this thesis is not the actual errors made in forecasting the economy but the way in which they are interpreted. The issue addressed in the rest of this section is thus the extent to which these events force the proponents of the devaluation story to give up or modify their more general model. In other words, can 1993 be explained as merely a particularly odd instance of the devaluation cycle in which the combination of external events was such that the inflationary effect was unusually muted? If this is the case, then the factors which lay behind the longer term pessimism of the Devaluationists still remain.

The rest of this section thus analyses how those members of the Panel which used the devaluation framework to structure their forecasts explain their performance, the effect it has had on their models and reflect on the policies they suggested. Taking first the figures for growth, which the Devaluationists generally underestimated, what do they see as the reasons for this? In terms of the differences between the numbers forecast and those actually recorded, Andrew Britton is in the enviable position of having very little to explain:

Britton [In February,] the Institute was at the top end of the Panel range with a forecast of 2 per cent growth ... This forecast now appears to have been exactly right ... The Institute's projections of domestic demand were also substantially correct, with consumer spending rising a little faster than we

expected (despite the Panel's worries about debt ratios) and fixed investment picking up a little.[7]

For others, however, there was more to explain. The revisions to the national economic data made in September were seen by several of the Panel as an important factor. As discussed in Chapter 7, one of the implications of this new data set was that forecasts for GDP growth, including that made by the National Institute, ought to be increased. However, due to their doubts about the trade data, the National Institute were reluctant to change their forecast which was, in any case, starting to look fairly accurate. Thus, Andrew Britton explains that, at the National Institute:

Britton What we tended to do was think that, probably, the provisional output figures were correct and the trade figures and stockbuilding were wrong. So when we got the figures with more growth in ... we put that in as a preliminary outturn [for net trade] because that was the published figure, but we offset the effect on stockbuilding so that we weren't changing our GDP forecast ... So when I said we hadn't made any changes, I meant we hadn't made any changes to output. But obviously we had to take on board the provisional figures for the first half of the year, even though we regarded them with some suspicion.[8]

On the other hand, for those with lower forecasts, the data revisions offered something of an explanation. For example, David Currie concedes that the London Business School 'under-estimated the extent and pace of recovery during the year,' but argues that:

Currie In large part, this was due to the substantial revision to the estimates for GDP in 1992. Instead of the weak numbers first estimated, the revised figures (including rebasing) show that GDP was at its lowest in the first quarter of 1992, and was rising slowly throughout the rest of the year. With that starting point for our forecasts in 1993, our view of growth during the year would have been more positive.[9]

Another important factor in understanding the London Business School original forecasts is the judgemental adjustments which were made. Clearly, with hindsight, some of these can be seen as mistakes. However, what is interesting is that Currie believes that these mistakes of judgement were, at least in part, due to the forecasting team giving too much weight to the views of the members of their Consortium Panel, in particular IBM, when revising their provisional forecasts.[10]

In contrast to Currie, Wynne Godley does not seem to think that the data revisions had a significant effect on his own forecasting performance. This is perhaps due to the fact that he has a larger discrepancy to explain, but even so, he does not mention them as even a contributory factor. Rather, Godley highlights the unusual way in which export prices rose following the devaluation:

> *Godley* It was quite out of line with past experience that export prices rose so much following the devaluation with the result that there was no deterioration in the terms of trade and therefore no J curve; this unusual change in export prices probably improved the balance of payments by £3-4 billion. At the same time, the rise in import prices does not seem to have fed through to consumer prices, and therefore did not reduce real personal disposable income. This is one of the reasons why my forecast of consumption was too low.[11]

In addition to these price effects, Godley also identifies two other factors which he did not anticipate:

> *Godley* One thing that I did not foresee was the stock exchange boom, which I think was quite unexpected. [Secondly]. I didn't really take into account the extent to which households in debt would be alleviated by having lower interest rates. So there are two factors which, had I anticipated them properly, then I would have got it [1993] a bit better. But I was particularly surprised that I was wrong.[12]

However, although these misjudgements have been made, the key question in this context is the extent to which they mean that Godley's underlying model needs to be changed. In other words, how consistent are the observed events with the theoretical model? In the case of 1993, this task appears more complicated because the theoretical model was applied under very different economic circumstances to those in which it had been developed. However, in every year these issues will arise to a greater or lesser extent and form part of the basic problem of economic forecasting. In 1993, the key judgements related to the effects (if any) the unusually high levels of private and corporate debts, which had created an unprecedented financial surplus, would have on the way the economy responded to the devaluation? Thus:

> *Godley* Following the slump there was an unusually large private sector surplus which I take to be the consequence of people having acquired debt and then trying to repair their balance sheets. This has happened to both the

household sector and the corporate sector ... And therefore they had very low expenditure relative to their incomes. The position was quite abnormal - nothing like it had ever happened before - and the forecasting problem was to know when this financial surplus would fall.[13]

It is this issue of the timing of the fall in the private sector surplus which Godley believes that he got wrong:

Godley I took the view that it would take a year or two longer to start falling than it actually did. But it was always a great uncertainty to me and I was very unsure whether I was correct, because it might fall earlier - I just did not know, and I had no past experience to guide me.[14]

The next stage in this process of self-analysis is to reflect on what has happened, to compare it with what was expected and how and where mistakes were made. Again, Godley's response is most candid:

Godley I am not yet sure whether or by how much I shall want to change my model in the light of some of the things which happened in 1993 (in particular to inflation and unemployment) which were surprising to me. There certainly seems some evidence of increased flexibility in a number of areas.[15]

Thus, Godley is willing to consider the possibility that the events were not just a bit of 'good luck' for the UK economy and that structural relationships within the economy might have changed in some ways. However, it seems that what Godley is referring to here is the weight which should be given to certain factors and, more importantly, the time frame over which they now act. Thus, the changes he considers are more or less at the margin of his model. In terms of the 'big story' he remains adamant that he has got it right:

Godley I have always emphasised my strongly held view that the main objective of forecasting is not to get the short term numbers right (which is obviously impossible to achieve reliably) but to identify the major strategic issues facing the country and reach conditional conclusions about the proper policy response to alternative outcomes ... I have, in particular, been absolutely clear ever since the Panel started up that personal disposable income and demand were in need of 'rebalancing' on a large scale and that this would require substantial fiscal restriction at some stage ... Nothing happened in 1993 to alter my opinion that sustained recovery depends upon achieving a faster growth in net export demand. So while, as I remarked a year ago in

> my first report for the panel, 'The government is essentially on the right track' there is still a very long way to go before the weaknesses of the economy, accentuated by so many policy errors during the last twenty years, are set to rights.[16]

Thus, Godley's surprise is not so much over the *events* which have occurred, but the *time* at which they occurred - in general, consumption started to rise sooner than he had expected. However, although he was surprised that the rise happened so soon, this does not challenge anything fundamental about his understanding of the economy:

> *Godley* I haven't really changed my model, or my way of thinking, at least not about income-output determination. It is just that I didn't get the timing of it right.[17]

A similar question obviously applies to the rest of the Devaluationists who must also ask themselves how much the economy has changed, and examine the extent to which their own models and theories are still useful representations of the world. In particular, they need to re-examine the ways in which prices are determined within their models and the extent to which labour markets have changed as a result of the Conservative government's policies.

As for Godley, some the most pressing questions are those which relate to net trade and to prices. However, rather than accept that the trade figures reflect any particularly significant change in behaviour, Britton and Currie take issue with this very assertion. For example, by emphasising the changes in the way the trade data have been collected, and uncertainty which surrounds it as a result, the urgency of the problem is reduced. Thus, one way in which the 1992 devaluation can be distinguished from, say, that of 1967, is through this change. As noted in Chapters 6 and 7, one result of these changes is that the trade data are not considered trustworthy by several of the Panel.

In addition to the uncertainty introduced by the new methodology, Britton also emphasises the difference between the *value* of imports and exports and the *volume:*

> *Britton* It is much more difficult to say what happened to external demand, since the uncertainty over the growth of exports and imports in the first half of last year is still to be resolved. The balance of payments has turned out much better than I expected, but that appears mainly to reflect a more favourable outcome for the terms of trade, rather than for net trade volume. The

recorded growths of export and import volumes are both much lower than expected.[18]

This further problematises the simple assertion that the net trade figures were unusually good, and seems to imply that the turn-around in trade performance is, at least in part, artifactual. In other words, in terms of the actual goods exported, there has been less of an increase than might have been expected given the devaluation and that exporters have exported less but earned more.

The other problem which the Devaluationists had to explain in 1994 is the surprisingly low inflation recorded in 1993. Both Britton and Currie accept that the devaluation has not had the inflationary effect they had anticipated:

Britton The rate of inflation, on any measure, has turned out rather lower than expected. For the fourth quarter of 1993 the outturn for the retail prices index (excluding mortgage interest payments) was 2.7 per cent as against a forecast of 4.6 per cent by the Institute last February, and an average forecast of 3.9 per cent by the Panel as a whole.[19]

Currie On inflation, we substantially overestimated the extent to which the devaluation of sterling would raise inflationary pressures in the economy during 1993 and the early part of 1994.[20]

However, despite this neither of them feels that it is necessary to alter his views fundamentally. Each one can identify special or unusual features of 1993 which, when fully appreciated, explain why this particular instantiation of the devaluation process has worked out in this particular way. Britton first re-emphasises the fact that it might be too early to judge how the devaluation has in fact worked because of the implausibility of the data:

Britton For 1993 as a whole import unit values (if the figures can be believed) rose by about 8 per cent, but unit labour costs by only about 1 per cent.[21]

Later in his submission, Britton returns to the topic of inflation and argues that wage behaviour during the recession and also in the period following it has been rather unusual. Britton bases his argument on the values of the 'residuals' in the wage and prices equations in the National Institute model. In terms of the framework adopted in this thesis, the size of the residual (i.e. the difference between the actual value and the computed value) is a sort of quantitative indicator of the extent to which the particular instance fits the more general story. Thus, the smaller the residual the more typical the

instance and vice versa. By examining the residuals produced by the National Institute model, Britton is able to construct a table of numbers which:

> *Britton* Confirms that wage inflation in particular was, relative to our model [i.e. general story], 'unusually' high in 1990 and 1991 ... By last year [i.e. 1993], however, the residuals in the wage equation ha[d] changed sign, suggesting that the wage increase was, relative to our model [i.e. general story], 'unusually' low.[22]

In addition, Britton is able to identify specific events in the economy, as well as mis-perceptions amongst the public, which explain why wage inflation in this particular economic cycle has differed from previous patterns. In the case of the unexpectedly high wage inflation of 1990 and 1991, Britton suggests that this is, in part, 'because the depth and duration of the recession was not at that time fully appreciated by wage bargainers.'[23] Later, as the economic cycle turned and the recovery was accompanied by lower than expected inflation, Britton argues that may have been because

> *Britton* Weight was being given to the very low figures for the headline RPI [inflation] figure which does not feature in our model, because of pay restraint in the public sector or because the labour market was slow to recognise the turning point in unemployment.[24]

Currie follows a similar line, firstly recognising the surprising absence of significant inflation which followed the devaluation and then identifying specific events and factors:

> *Currie* The fall in inflation following devaluation has been most welcome, and has led us to re-examine our view of prospective inflation. In part, current low inflation reflects certain special factors [e.g. falling mortgage rates, discounts and sales in shops], the robust policy of cutting the large fiscal deficit, as well as low inflation in the rest of the world, reflecting the level of competition in global markets.[25]

In this way, Britton and Currie maintain their general model as an explanatory structure by introducing special factors into the processes which mediate economic activity e.g. the misguided public. However, as noted above, there is also the argument that the low inflation figures reflect a more fundamental shift in the economic structure and that the Devaluationists actually have the wrong model. Currie recognises this as a candidate explanation but, ultimately, restates his faith in the original analysis:

Currie There is also the possibility that [the low inflation] reflects changes in the structure of the UK economy leading to improved flexibility of supply performance ... although we accept this explanation in part, we retain some scepticism as to whether this improvement will be sustained. We therefore see some risk of rising inflationary pressures as recovery continues.[26]

Thus, in conclusion we can see that, though the identification of transitory and contingent factors, the low inflation of 1993 is explained and accounted for, but the validity of the general model, and its implications for future inflation are also maintained:

Britton There have a been a lot of special factors that you can identify in the rate of inflation over the last year, particularly in food and household durables, where there have been a lot of price cuts, particularly in the sales. And I think that I am not really saying more that, perhaps, there won't be so much of that in the future.[27]

The 'Classical' Economists

For the 'Classical' Economists things were rather different. They had been more optimistic about the future than the 'Devaluationists', forecasting that growth with low inflation was the most likely outcome over the next few years. Arguably this is what had happened and so they were able to claim that they had more or less got it right all along. Thus, for example, Tim Congdon argues that:

Congdon The February 1993 [Lombard Street Research] *Submission* [to the Panel] argued, on the basis of an equation using the output gap concept, that 'above-trend growth can be reconciled for several years with low inflation' and that 'the next few years should be good ones for the British economy' ... Much of this forecast, including its generally optimistic tenor, has proved correct.[28]

However, Congdon does acknowledge that his forecast erred in three inter-related ways, namely:

Congdon 1. it under-estimated growth in 1993;

2. it failed to foresee the fall in unemployment which did in fact occur, and

3. it failed to forecast the acceleration in broad money growth in late 1993.[29]

Like Currie, Congdon believes a large part of the under-estimate of GDP (about half in his case) can be attributed to the revisions to the economic data. However, the rest of the error is due to a second and more 'fundamental' reason - the rate of growth in the second half of 1993 really was faster than he had expected. In this respect, Congdon is now in the same position as the Devaluationists were over their forecasts for inflation. He must now identify some particular events which explain why this was if his general model is to be maintained. Thus, Congdon argues that the lower interest rates had a more significant effect than he had anticipated:

> *Congdon* The failure to fully foresee the strengthening of activity in the second half may have been due to exaggerating the tightness of the constraint implied by low monetary growth. In practice, the drop in interest rates may have been responsible for people trying to switch out of interest-bearing deposits into more rewarding assets, such as equities and real assets (including consumer durables and cars), on a greater scale than had been anticipated.[30]

Of course, the idea that economic agents will try to maximise their returns is not controversial and so Congdon's account maintains the general model. On the other hand, he does have to admit that the scale of the transfer into other assets was, in this instance, unusually large.

On unemployment, Congdon argues that relatively low productivity growth in 1993 (itself an unusual event), when combined with the higher than expected growth, led to a bigger fall in unemployment than he forecast. Finally, on the under-estimation of monetary growth, which is a crucial part of his economics, Congdon is keen to point out that the mistake was not due to forecasting credit trends wrongly. Rather it is attributed to 'neglect[ing] the potential capital in-flows into the non-bank sector from abroad'[31]. As a result, the mistakes actually *confirm* the general model:

> *Congdon* The faster-than-expected monetary growth in late 1993 contributed to the improvement in balance-sheet strength across the economy and so the greater-than-forecast buoyancy in domestic demand and employment ... The message of [this] is clear ... 'in any worthwhile discussion of the macroeconomic situation, we must make an attempt to relate the demand to hold broad money to the quantity of such balances actually in existence'.[32]

For the other member of the Classical camp, Patrick Minford, the situation was perhaps rather different. In February 1993, he was at once both a pessimist and an optimist. As far as 1993 was concerned, he was one of the more pessimistic economists on the Panel,

forecasting low GDP growth and advocating large and immediate cuts in interest rates in order to ensure the recovery. Over the longer term, (i.e. into 1994 and beyond) he was one of the more optimistic Panel members, forecasting strong growth accompanied by low inflation and falling unemployment. Thus:

> *Minford* At the start of 1993 we expected modest inflation for the year followed by a decline to around 3 per cent; growth weak in 1993 rising to 3 per cent in the following years because of supply-side improvement (particularly greater labour market flexibility); and unemployment remaining high in 1993 but falling steadily thereafter for the same reason. Our fear, reflected in our forecast, was that demand weakness produced by the previous extreme tightness of money would delay recovery and the growth unleashed by the better supply side.[33]

Of course, this is not what happened - GDP growth in 1993 was nearly 2 per cent more than Minford forecast in February of that year. In his retrospective analysis, Minford identifies three factors which led to the unexpectedly strong recovery:

> *Minford* There seem to have been three simultaneous and probably connected sources of greater strength: in net exports, recruiting and in consumption. Our model may have underestimated the exchange rate effect on net exports and the real interest rate effect on consumption; and the lags in the unemployment relationship may have speeded up.[34]

However, in interviews it became clear that there was an additional reason why the forecast produced in February had been so low. Minford pessimistic short-term forecast was actually the result of his own personal pessimism. The Liverpool economic model had initially produced a higher forecast but Minford and his team had 'restrained' it. The reason for this adjustment was uncertainty over what to do about the unusually high levels of debts. In Minford's case, this uncertainty was compounded by additional problems of how to interpret the 'regime shift' caused by moving from a policy in which the exchange rate was fixed to one in which it was allowed to float:

> *Minford* Had [we] been in a floating regime all along, one would probably have let the model roll. The difficulty was how to interpret this post-ERM regime shift ... I think we felt that the sharp difficulties with balance sheets - the overhang of the ERM really - wouldn't go away quickly. They are not explicitly in the model, these balance sheet factors, these things like negative equity and so forth. So we thought there was going to be

> something of that still in there ... Basically we thought that it would take 1993 to work out of the system and we also assumed that there would need to be interest rate cuts during 93, quite a lot of interest rate cuts. In fact, they obviously worked out of the system quicker.[35]

Thus, like Godley, the high levels of indebtedness in the economy caused Minford and his colleagues difficulties. Also like Godley, they over-estimated the time period over which this unusual situation would have a significant effect on the economy. However, like his monetarist colleague Tim Congdon, Minford's forecast for low inflation was proved to be more or less correct, despite the higher than expected growth and so in this respect he had few worries. As he observes, it was always his view that:

> *Minford* Policy had massively deflated the economy so that the faster slowdown in inflation was not too surprising: we somewhat underestimated it in the short-term, allowing too much for the temporary exchange rate effect on inflation. [But] the evidence has been consistent with our view that the UK supply side has improved and that monetary policy has been highly deflationary.[36]

In other words, the lower than expected inflation is interpreted as proof that the more general story Minford wants to tell about the importance of monetary influences and the flexibility of markets is confirmed by the data. Thus, he sees in 1993 yet more evidence that the Keynesian Devaluationist story, with its rigid labour markets and high inflation is inadequate for the task:

> *Minford* The claim of the people that I have been arguing with, has been that there will be inflation, much higher inflation - that any sort of growth would set off inflation. After all, we had an argument before all this about leaving the ERM. People like Andrew Britton and David Currie, and Gavyn Davies come to that, argued that it would be highly inflationary to leave the ERM. I mean, it is quite clear who won that argument.[37]

Finally, with regard to policy, even as 1993 progressed and it became evident that growth was going to be stronger than Minford had originally anticipated, he never wavered from his original view that interest rates ought to be cut. In this respect he differs from Gavyn Davies who, although advocating interest rate cuts in February, did not do so in July or October. Moreover, in February 1994, he still thought that this was good advice. Part of the reason for this is that the factors which combined to produce the better than expected growth could just have been due to chance:

Minford These developments amy [sic] just have been random 'errors', the result of a lucky draw, a positive event influencing all three sources ... Had there been an unlucky draw then the recession would have been yet more prolonged, unnecessarily given the already savage deflation in the system which should have been evident from normal economic theory (instead much was wrongly made of the supposed 'inflation' stemming from a downward float) ... In summary the government have been fortunate that a still poor situation of delayed recovery is not worse still.[38]

However, to say that because the government did not cut interest rates and were 'lucky' enough to get GDP growth of 2 per cent hides a more fundamental question in Minford's view, namely:

Minford What you have to ask is whether you could legitimately have expected to have a robust recovery with those real interest rates. That is really the question, and I think that they were lucky to get away with it. But they haven't really got away with it because it is still a very weak recovery. I suppose another way of putting the point is, suppose I had correctly forecast 2 per cent growth, I would still have taken the same policy position.[39]

Thus Minford, even though GDP growth of 2 per cent has been recorded without the interest rate cuts he believed were needed, still maintains that his original diagnosis was correct, and moreover that events have shown it to be so.

Minford We were obviously forecasting lower growth on the grounds that there were depressive forces in the economy. Clearly they weren't as depressive as we thought. But on the issue of inflation, it is quite clear that with growth coming in at what these others expected, but accompanied by very much lower inflationary pressure, then on that we were clearly right ... the significant fact has been that inflationary pressures have been very much lower than expected and that is why interest rates have been continuously cut. And that points really to there having been a significant over-estimate of the inflationary dangers.[40]

Summary

From the above discussion, we can see that neither the Devaluationists nor the Classical felt that the events of 1993 fundamentally challenged their basic ideas about how the economy worked. For nearly all the Panel, GDP growth had been higher than they had forecast at the beginning of the year. However, this was largely explained by two sets of factors. The first of these was the revisions to the data which took place in the

Autumn. Had this data been available at the beginning of the year then their forecasts for growth would have been as much as 0.5 per cent higher.

The other factors identified related to the unusual nature of the economic situation in 1993. Thus, many of them mentioned the uncertainty created by the unusual set of economic circumstances which was the result of the preceding economic cycle. For most of the Panel, the unprecedentedly high levels of indebtedness were the principal source of this uncertainty. For Minford there was, in addition, the effect of the change in policy. In either case, however, we can see that, in those circumstances where there is no historical precedent, the forecasters have their biggest problems. However, and perhaps ironically, in this case it was Andrew Britton, the forecaster who made the most of historical precedent, who produced the best forecast for demand.

In addition to these shared concerns and problems, there were also explanations and discrepancies which related more directly to specific forecasts. For the Devaluationists, the problem was that inflation had been much lower than they had expected given the growth which had occurred. They tended to explain this as being the result of special factors like price-cutting in high street stores, mis-perceptions of the depth/duration of the recession and so on. However, they also argued that because these effects were temporary in nature, their underlying model, which was the basis for their longer term pessimism, was untouched. As a result they remained pessimistic about the future despite the events of 1993.

For the classical economists, Minford and Congdon, the problems were much less severe as they had forecast low inflation in the first place. However, it is difficult to conclude from this that they had the best models. As we have already seen, the Devaluationists were able to identify specific events which, in this particular instance, mitigated the inflationary impact of the devaluation. It is only over the following years that the 'real' test of the theories emerges. On the one hand the Classical economists foresee persistent low inflation and above trend growth. On the other, the more Keynesian Devaluationists see growth constrained by inflationary pressures due to hysteresis in the labour market and an increasing balance of payments deficit. However, as we have seen throughout this thesis, these stories have to be interpreted in the context of the particular situation in which the economy finds itself, the policy decisions taken by government and the spending/saving decisions of economic actors. As a result it is

highly unlikely that 1994 will prove any more decisive than 1993 in distinguishing between the good and the bad in economic theory.

Debates and Controversy

General Cases and Economic Narratives

As we saw in the previous section, analysing forecasts for economic growth in 1993 was, in some ways, similar to the classic SSK 'controversy story' in which the meaning of empirical data is ambiguous and contested. It was accepted by everyone that the UK economy had been through a severe recession in the previous 2-3 years. What was not so clear was what would happen next. One view was that the UK economy would continue to grow, with inflation and unemployment falling. The other was that the recovery would be little more than a brief flurry of activity followed by a much longer period of slow and difficult growth against a background of high unemployment and the re-emergence of inflation. The two sides to this controversy were thus the economic optimists and the economic pessimists. However, it would not be fair to say that the two camps were homogeneous - there were differences *within* them as well as similarities *between* them. This section thus begins by unpacking the two sets of stories a little more carefully.

Both the optimists and the pessimists had a general story or theory which explained the current situation and provided forecasts for the future. The pessimists based their forecasts on the effects of the devaluation which came about after the UK's exit from the European Exchange Rate Mechanism (ERM). For the optimists, the picture was rather different. To start with, they tended see the ERM as having been the problem rather than a potential solution. Thus, having left the ERM, there was nothing between the UK and economic recovery. In this optimistic interpretation, the story was that the economy was a long way from equilibrium. As a result of the ERM, unemployment was too high and output was too low. Consequently, the next few years offered the pleasing prospect of output rising and unemployment falling, all without the emergence of any significant inflation.

In the devaluation story, the argument was that, in previous devaluations, the gains from the more competitive exchange rate were offset fairly quickly by rising prices. As a result of this experience they argued that the prospects for the UK economy after this devaluation were not especially good. In particular, inflationary pressures were likely to increase as a result of the devaluation itself and these would be

compounded by hysteresis in the labour market and the increase in output itself. This in turn would require deflationary policies (to keep inflation down) and, as a result, economic growth was unlikely to be fast enough for long enough to reduce unemployment significantly. The pessimistic picture therefore was of an economy trapped between high unemployment and high inflation.

Godley's variation on this theme was that it was the UK's tendency to import more than it exports which is the problem. If growth is not warranted by net trade then the Balance of Payments deficit becomes a binding constraint, requiring deflationary policies to contain it. The result however is still the same - low growth and high unemployment are the most likely future.

These then are the general stories which the Panel of Forecasters told: one was a story about a devaluation which occurred in 1967 and its consequences; the other was the classic equilibrium story of an economy returning to it's 'natural rates'. However, although these stories provided the template for understanding of events, they did not prescribe exactly how the story will be told. Each story is made up of several parts, includes many different actors and the roles given to (and taken by) each varies with each instantiation. The skill of economic forecasting is thus in understanding the particularities of the current version, the way the elements of the story combine and then using these to predict what the outcome will be - to guess the sting in the tale! This is where the expertise of the economist as a socialised member of both the economic and wider community is important. An econometric model is a tool which reduces and removes the atypical and the unique in order to identify the average and the regular. Like the economic narrative of 'devaluation' and 'equilibrium' it is something which can be used to structure the story but cannot actually tell it.

In 1993, the tensions between the 'general' case and the 'particular' instance manifested themselves in several different ways. For some the tensions were small - Andrew Britton relied extensively on the 1967 devaluation in the analysis of, and forecasts for, the UK economy in 1993. Thus he saw the differences between the general case 'devaluation' and this particular instance of it as being minimal. The same is also true for Tim Congdon, although he told an 'equilibrium' story rather than a devaluation one. For others, however, there were some differences. For example, Gavyn Davies, although basically backing the 'devaluation' story, argued that the economy was actually much weaker than in previous cases as a result of the high levels

of indebtedness. Thus, interest rates could (and should) be cut still further before the inflationary pressures predicted by Britton and Currie would be seen. The tension is not over whether a devaluation has occurred - it clearly has - but whether, under the current circumstances, the typical effects, as exemplified in the 1967 devaluation, will be seen. Britton and Currie say they will, Davies argues that they will not.

This happens within both the optimist and pessimist groups. In each case we can find forecasts based on precedent (Britton and Currie for the pessimists, Congdon for the optimists) and forecasts which emphasised the unique nature of the current combination of events (Davies, Godley and Sentance for the pessimists, Minford for the optimists). Thus some of the differences between the Panel's forecasts can be explained by their interpretation of the current situation. Britton and Currie, who probably have the largest econometric models, were the ones who paid the most attention to historical precedent, (i.e. previous devaluations) in grounding their forecasts. Consequently, they tend to minimise the importance of the things which make 1992-93 different from before. As a result, the positive impact of the devaluation tends to dominate the forecast in the short term and the inflationary effects in the longer term. This is very different from, say, Godley who paid much more attention to what made 1993 unique (e.g. the unprecedentedly high levels of private sector debts) in making his forecasts. As a result, he is much more sceptical that the devaluation can have a significant impact.

However, there is not a simple relationship between focusing on the poor state of the UK economy (i.e. the particular) and putting a more pessimistic spin onto the general story. In Minford's forecast, it is precisely the features which make 1993 different which ensure the recovery over the longer term. For example, Minford believes that the labour market has been transformed by policies which have reduced the natural rate to about 1 million, and that global competition, together with a sea-change in public opinion, have made inflation a thing of the past. And so on. As a result of this, even though the UK economy is in an exceptionally poor state in early 1993, provided the government does not make any more mistakes, its prospects are good.

Although the focus so far has been very much on the grand narratives which cover the whole economy, it can also be seen that the Forecasters saw in the current data different versions of the same story. Thus, how the devaluation will turn out depends on judgements of similarity and difference between this devaluation or recession and previous ones. For some it is essentially the same, and so can be forecast reliably using

econometric models. For others, however, there are important differences between 'then' and 'now' which colour and change the way the story unfolds. Thus, for example, unprecedentedly high levels of debt restrain consumers' expenditure, thus muting the effect of the devaluation. Alternatively, the labour market policies enacted during the 1980s mean that the UK economy can recover much faster, and with lower inflation, than in the past. In this way, the judgement about what is similar and what is different feeds into the structure provided by the general case and transforms it from, say, a 'devaluation-in-general' to 'this-devaluation-in-particular'. Of course, the general case is also the macro-econometric model and the use of residual and other adjustments is thus understood in this context as the addition of expertise to extrapolation.

Particular Instances of Economic Theories

In this section I continue to develop the idea of economic forecasting as being the application of generalised and/or stylised 'cases' to events in the world through the apprehension of similarity and difference. Unlike the previous section, which focused on what might be called the 'big stories' of economics, the focus here is on the more minor and localised controversies which emerged during the Panel's meetings and in my interviews. However, like the previous section, the focus of the discussion is on how a particular set of data is interpreted as being an instance of a more general case rather than its refutation. The discussion draws on the Panel's views on unemployment, Public Sector Borrowing and the usefulness of monetary indicators

Unemployment

Taking first the debate over the unemployment data. The debate was precipitated by the unexpected falls in the number of people claiming unemployment benefit in the first half of 1993. This was not anticipated by the Panel members and meant that they needed to identify some way in which 1993 differed from previous post-recession periods in order to explain why unemployment had fallen so rapidly and so soon. In other words, what needed to be established were the particular features of 1993; the features which, when acknowledged, will both preserve the general model (be it hysteresis or classical) and also account for the discrepancy between that model and the economy. As was discussed in Chapter 6, several hypotheses were put forward ranging from an overly-pessimistic view of economic prospects on the part of employers in the latter half of 1992 to the suggestion that nothing had happened at all in the economy at all and that the fall was the result of DSS policies. By examining these ideas, it was shown how, through the identification of novel events, general propositions about

unemployment can be maintained in the light of contradictory experiences. Thus a range of non-economic factors were considered as possible explanations, although none was persuasive to every one.

In addition, the discussion in Chapter 6 also referred to the controversy over the importance of hysteresis which, in many ways, sets the framework within which typicality and particularity were assigned. After all, what you think ought to have happened in the first place plays an important role in framing the question of what needs to be explained, what is likely to effect the outcome and so on. In the case of unemployment, the rapid fall was less of a problem for Minford than the rest of the Panel as it was closer to what he had expected to happen.

By the time of the October meeting, the falls in unemployment had stopped and the passage of time had brought a closure of the debate over the causes of unexpected falls in unemployment. Part of the solution to the controversy lies in the new statistics - part of the original problem was that the unemployment figures had fallen earlier in the economic cycle than in previous post-recession periods. In other words, the unemployment data did not look much like an example of the general case. However, the new data, moved the start of the economic recovery back in time. This means that the falls in unemployment occurred closer to their usual place in the cycle and this particular post-recession period is now more easily recognised as an example of the general case.

However, a slight complication remains as the rate at which unemployment fell was faster than is usually the case and therefore does not fit the general pattern. It turns out that the special or unusual factor which explains this is the rise in unemployment which took place in the months before Christmas 1992 when there were no economic data to suggest recovery. Thus, the changes in the economic data enable the economists to re-assert the link between the economic cycle and unemployment by attributing the deviation from the general case to a loss of confidence in the business community which caused unemployment to rise above what it should 'really' be. In 1993 the 'mistake' is corrected and the unemployment data are returned to their more usual relationship with output.

Public Sector Borrowing Requirement

The second issue which was important in the Panel's meetings was the Public Sector Borrowing Requirement and the extent to which it was cyclical (i.e. going to go away

on its own) or structural (i.e. going to persist unless additional tax and spending plans were announced). As shown in Chapter 6, this debate exposed the fragile nature of the consensus reached in the February meeting, at which six out of the seven had agreed that fiscal policy should not be tightened during 1993. By moving the debate into 1994, those Panel members that wanted further policy action had to develop additional arguments to support their claim. They did so by linking the policies necessary to ensure a sustainable PSBR to other economic and political issues, thereby creating a stronger case for the policies they believed to be necessary.

In July, after the Chancellor's March Budget had introduced the sort of post-dated tax-raising measures some of the Panel favoured, the Panel returned to the topic of the PSBR again. Although the PSBR would be expected to come down in the recovery phase of the economic cycle (another general story) the exact amount (i.e. the way in which this particular instance would turn out) was difficult to forecast. As a result, some of the Panel did not believe that the tax and spending reforms announced in the March Budget had done enough to ensure that the PSBR would be reduced sufficiently. However, those Panel members that did think this tended to have to reinforce this argument by drawing on other economic stories and reasons. Thus, the weak case for arguing that this particular instance of the general case 'PSBR-reduces-as-economic-growth-resumes' was bolstered by arguments that policies to ensure the PSBR's sustainability could simultaneously have favourable effects elsewhere. In other words, attempts are made to minimise the importance of the uncertainty which surrounds the particular value of the PSBR four years in advance and to articulate other reasons why fiscal measures which could help to ensure sustainability of the public finances are desirable.

One such example is the 'Chancellor-cuts-taxes-and-ensures-election-victory' story in which it is suggested that tough fiscal policies now can, if proved unnecessary, create the space for tax cuts later. (They would also avoid the nightmare scenario 'Chancellor-raises-taxes-and-loses-election' as well!). The other way in which the PSBR was linked to other issues was via the case of the 'twin deficit' problem. The argument here was that, if past patterns were repeated, then increases in GDP sufficient to bring down unemployment much below 2 million would increase imports, and hence the trade deficit, to such an extent that interest rates have to rise to prevent a 'sterling crisis'. On the other hand, if GDP growth (and thus imports) were to restricted such that the trade deficit did not become a problem, then unemployment would not fall at all and

the PSBR would continue to grow. The 'twin deficit' case is thus very closely related to the 'devaluation' story and also draws heavily on historical precedent. The important question therefore is how does the particular set of conditions which define the UK economy in 1993 shape this story?

Minford and Congdon, the proponents of the 'Classical' story see the twin deficit problem as something which is not really a problem at all. However, as Wynne Godley points out, for this to be the case they are, implicitly, making the claim that this particular instance of the general 'economic-growth-creates-trade-deficit' case will be very atypical. More specifically, they 'are (must be) assuming a growth in net export demand far in excess of anything that has ever occurred in the last fifteen years'.[41]

In contrast, those who think that the general case will present itself in a much more usual fashion, and are thus forecasting a widening trade deficit, are asserting that the general patterns of the past will, more or less, be repeated. They also believe that this is a problem. Thus, Godley and the more mainstream economists on the Panel believe that a poor export performance and a high propensity to import is a serious weakness in the UK economy. Moreover, they agree that it is something about which governments ought to (and could do) something about. The problem is that the 'balance of the economy' is wrong. In particular, too large a share of resources is devoted to consumption with the result that net exports and investment are too weak. The policy solution is an increase in taxation which reduces consumption, can be used by the government for investment and also allows interest rates to fall, thereby boosting both exports and private sector investment. This is all quite independent from the sustainability of the PSBR, although the policy is, in many ways, the same. Of course the specific policies advocated varied but one thing which they all shared was that these were long term policies which required a change in the political culture if they were to succeed.

In fact, five of the seven Panel members felt that net exports were too weak and that some remedial action was necessary, whereas only four out of the seven felt that not enough had been done to secure the sustainability of the PSBR. There was therefore a fairly clear majority for policies to improve competitiveness and hence net exports (increased training etc. for most, increased demand for Godley). In addition, there was a weaker majority for a further tightening of fiscal policy, with Andrew Britton agreeing that it would be understandable if taxes were to be increases.

In summary, three separate interpretations of the public finances were thus on offer in July. The first was that there is not a problem at all and that low unemployment, a balanced budget and acceptable trade figures are to be expected. This argument rested on an unusually good performance by UK exporters. The second strand is that the fiscal tightening proposed in the Budget was not sufficient to ensure the sustainability of the PSBR and that additional measures were needed. This argument rested on the claim (highly uncertain) that the PSBR was such that, even with the policies already announced, it would remain stuck above 3 per cent of GDP. The third strand of the argument was that, even if the PSBR situation had been adequately dealt with, there were other reasons why further tax increases were desirable. This last argument drew on the experiences of the UK economy of the preceding 10-20 years and argued that if past performances were repeated then any recovery in the UK economy was going to be very lacklustre indeed.

Monetary Indicators

Finally, the importance of monetary indicators was also discussed, although it was not something the Panel's reports actually devoted a great deal of space to.[42] Once again, the aim of the analysis was to show how the art of economic forecasting lies in interpreting how the particular set of circumstances which define the present moment will influence the outcome of the more general case of which the present is but an instance.

As described in Chapter 7, UK money supply figures were sending quite different signals in October 1993, depending on which definition of the money supply you used. 'Narrow' money (M0) implied GDP growth in the range of 2-3 per cent (and thus no urgent need to relax monetary policy), whereas 'broad' money (M4) was implying virtually no growth in GDP at all (suggesting that the Chancellor needed to act in order to ensure the recovery). The analysis showed how, for the mainstream economists, M0 is something which gives up-to-date information about one part of the economy and is used to corroborate their stories, forecasts and expectations in a fairly general way. However, because it is not central to the economic story told by the mainstream economists they are under much less pressure to account for the way in which the particular instance relates to the more general stories - M0 is something of a bit-part player and most economists have more important things to worry about.

On the other hand, a monetarist economist like Minford must explain how and why this particular set of money supply figures are part of a larger, more general case in which demand for money is systematically related to other aspects of the economy. To do this he weaves together a wide range of data and clearly this is intellectual work which Minford has to put in because his economics requires that the relationship between the demand for money and other economic data be fully explained.

The debate over broad money was somewhat different in that it was the mainstream modellers who now had to put in the extra work. They had to discount the demand for broad money which was, on past patterns, pointing to a very different economic future to the one that they were forecasting. In other words, in order to maintain their general story, the Panel members had to identify specific features which explained why the relationship between the demand for broad money and economic activity was going to be different in this economic cycle. Tim Congdon, on the other hand, was content to assert that the general analytic framework he had already articulated was quite adequate.

In other words, we can say that Tim Congdon sees the current situation as absolutely typical, with weak broad money supply growth leading weak economic growth. The majority of the Panel need to explain why the low broad money figures do not represent cause for concern, i.e. what it is that makes the current situation different from the general case. Thus, the non-monetarists join Patrick Minford in highlighting the role of the banking sector and the unusual lending spree of the 1980s which resulted in exceptionally weak balance sheets and massive debts. Although low by historical standards, they argue that, because banks are unwilling to lend and business is keen to exploit new ways of raising finance, it is quite possible that the money supply figures are consistent with continued and reasonable economic growth.

Once again, we can see how the task of economic modelling revolves around the interpretation of specific events as instances of more general categories. In the case of M0, it is the monetarist Patrick Minford which has to put in the most intellectual work in order to produce a coherent account of how the particular events of 1993 relate to his more general models. However, this is not sufficient to discredit monetarism per se, because when we look at broad money then it is the mainstream modellers which have to do the extra work.

Implications for policy making

This final section examines how economic forecasters can advise economic policy makers when, as has been shown, there seems to be no way of knowing either what will happen next to the economy, nor even of deciding quite how what has already happened should be understood. In situations of such chronic uncertainty, what is the value of bringing a (diverse) group of seven economists together and asking them to produce forecasts and make policy recommendations?

As noted in Chapter 7, one benefit of the Panel was the chance it gave them, as representatives of the economics profession to restore some public faith in the discipline. Hence, for example, Gavyn Davies's comment that:

> *Davies* I think that the Treasury has given us a massive opportunity, both as individuals and as representatives of economics outside the Treasury[43]

However, this section is really about the effects of the Panel's meetings on the economic theories of the Panel members and on the influence, if any, they have over the policy process. We have already seen that the economic outturns of 1993 were such that none of the Panel members felt that they needed to fundamentally change their ideas. In the rest of this chapter I want to examine how the Panel are influenced by each other and how, as a group, they can influence the Treasury and the Chancellor.

An important part of this is the departure of the UK from the European Exchange Rate Mechanism (ERM) in September 1992 which created the context in which the Panel was created. At the time, the exit from the ERM was widely regarded as an economic and political catastrophe, and was quickly dubbed 'Black Wednesday' by the media . As Alan Budd explained:

> *Budd* Associated with [leaving the ERM] was the feeling that the Treasury, in particular, had produced appallingly bad forecasts and that this was one of the reasons why we had made this ERM mistake ... So what the Chancellor does to, if you like, appease the wrath of those people who say he ought to be sacked ... and also that the Treasury forecasters should be sacked ... [is to say] 'Well, actually we never paid that much attention to our forecasts, and it isn't the only thing we do. But, just to demonstrate the extent to which we do take account on outside views, I shall have this Panel of Independent Forecasters and I shall let them supplement what my own guides tell us'.[44]

Of course, cynics could point out that this was simply a way of shifting attention away from the Treasury's forecasts and onto other people's forecasts, the result of which would just be to show that everybody gets it wrong most of the time. However, in practice this might not be such a bad thing - after all the Treasury forecasts were well in line with the majority of other economic forecasts right throughout the whole ERM experience. Consequently, their mistakes were also the mistakes of the economics profession as a whole and this is an important observation that should not be lost sight of.

There was also another aspect to the Panel's meetings and that was they actually talked to each other and the Treasury. Thus:

> *Budd* We didn't just read these people's forecasts, they would actually be here and discuss them with us ... Between them, they have got some very sensible ideas and they cover a range of views and they can raise issues with us that we have not thought of. So it is like a bit of a supplement to the Treasury, which is very worth having ... Equally, while we are trying to think what [policy] advice to give the Chancellor, we are listening to these guys and hearing what they have to say. It is like having six or seven extra officials at the Treasury, giving us their views [and] widening the range of opinion to which we are exposed and therefore, if you like, the range of opinion to which the Chancellor is exposed. Now of course the Chancellor could always read what they say in the newspapers, but putting them together does, I think, genuinely add something.[45]

These then are the antecedents to the Panel of Forecasters. The questions which remain are how does this discussion advance economics and how does it advance economic policy?

In the terms of economic science, the issue is whether through the discussions at the meetings, together with more informal contacts outside them, the Panel are able to advance economic theory, or at least its debate. Perceptions on the progress made on this front vary. For example, in the February 1994 Report, the Panel, perhaps in response to the general under-prediction of inflation in 1993, devoted a special section to this topic. Some thought this had worked well, others did not. For example, Andrew Britton commented:

Britton We actually had a good discussion of the theoretical basis on which we make our inflation projections which is reflected in the Report and in the Submissions that we did.[46]

On the other hand, other Panel member were less sure that anything very much had been achieved:

Minford The trouble was we didn't give ourselves enough time to discuss it in the meeting and so it was a bit of a mess in the event. I mean I had to do quite a bit to the draft to get it intellectually coherent and even that is not very good - there are still things which did not get finally incorporated. But it was a good idea to do it though.[47]

Congdon There was, in my view, a very unsatisfactory discussion about [inflation] and I wrote to Alan Budd and said I thought it was so perfunctory as to be worthless. I mean, if this is the best that the Panel can do on their different views on inflation, then, for Heaven's sake, we should be trying much harder.[48]

Nevertheless, the overall feeling seems to be that it is worthwhile for the Panel to focus their discussions on specific issues and try at least understand where and why they disagreed:

Congdon We might actually have a useful exchange of views, and I think that's, in a way, what the Panel should be doing ... It may be that just over working together that eventually it does all materialise. You see what I don't do, and what is very important for Godley, is a lot of work on ... net exports which I simply don't do. But nevertheless that work is quite useful and it does force one back onto trying to work out the relationships between different things ... And David Currie does a lot of work, and also Andrew Britton, on wage-price behaviour which is implicitly in my stuff but not played up very much. Gavyn Davies and Andrew Britton do a lot of work on the labour market which I don't really do.[49]

Thus, at a personal level, the economists on the Panel benefit from learning from each other and, as a result, a modest shifting of positions is possible:

Budd You might find [that] people will say, you know, 'Here is a really convincing story. It seems to fit, and this is how you should think about this'. And people might shift just a little bit, and we [i.e. the Treasury] would shift as well, and we would have made a little bit of progress.[50]

Although nothing dramatic should be expected:

> *Congdon* Will positions move? I don't know that they will very much.[51]

Thus, it seems unlikely that the Panel will succeed where the rest of economics has not and bring about an empirical and theoretical convergence towards a common specification for their models:

> *Budd* I think what I am more sceptical about is the empirical advances. I don't think that anybody is suddenly going to have a consumption function that works for example. I think our state of ignorance about this seems to be more or less constant.[52]

> *Minford* You would never get people to agree on every parameter, for God's sake! It is a very empirical business ... you get differences in detail specification. You always get different parameters.[53]

However, even if they cannot solve the problems of economic practice, can the Panel solve the problems of economic policy which are, after all, their main objectives. Certainly by emphasising the beliefs that they share the Panel can make a strong case for a particular policy recommendation, and this is a strategy which the Panel have moved towards as 1993 progressed:

> *Minford* In order to forecast you have to have your view about what causes things. And then also, in order to make policy recommendations you have to set out your reasons. So, therefore, insofar as we can reach agreement about what causes things, that would be helpful, wouldn't it.[54]

> *Britton* Now that we have had four meetings together we have got to know each others points of view quite well ... and the group has come to respect each others' positions rather more. If you remember last summer, there was a certain amount of exchange of letters in the press, which was reasonably friendly but was also a public criticism of each other. I think, whilst that could always happen again, I think at the moment the tendency is rather to try and find a common view, because we feel the Panel is stronger when it speaks with one voice.[55]

> *Budd* Before the last Budget, they, to a large extent, produced a consensus report. Now that in itself is very interesting, because they know that they are going to be more influential if they agree.[56]

However, from the perspective of the policy makers, one important issue remains - despite the efforts of the Panel to reach consensus, their views about the economy and the future remain different in several important respects. The question is therefore how to incorporate these forecasts into a single economic policy? To begin with we can note that taking the average of all the forecasts is not necessarily the best solution:

> *Budd* You might think that [forecasts] will be like shots at a target: [they] will blow the middle out with a few scattered around ... [In practice] the truth is liable to lie at one extreme or the other. It is a very peculiar business ... [So] taking the average may be the best thing to do in the long run, but year by year it is not the right thing to do.[57]

This, point is in fact perfectly illustrated by the Panel of Forecasters - in 1993 it was Andrew Britton's forecast for growth, initially an outlier, which turned out to be the most accurate. Thus, some way other way of dealing with the variation in the Panel's forecasts is needed if policy is to be based on the best advice. At one extreme, the Treasury could minimise the variation by taking only one forecast into account, at the other they could maximise it by giving equal weight to all the forecasts:

> *Budd* The Minford approach to having the Seven Wise Men would be that it is the Treasury's job to decide who is right ... and act accordingly. Another possibility is to say that any of them might be right. They can't all be right, but any one of them might be. So you conduct policy, because these are all sensible people, on the understanding that any of them could be right. That is another extreme - you try to make some sort of robust policy which would come out best.[58]

In practice however, when Alan Budd himself is giving advice he favours a third approach which is to weight the different forecasts in some way:

> *Budd* You already have a model of your own, you have your own a priori views and then you also weight these individuals according to how coherent their view seems to be and how good their forecasting record seems to be. You sort of lean one way or the other, so that you have a feeling that X or Y is most likely to be right and you attach less weight to extreme views.[59]

Of course, Alan Budd is not the only person giving advice, and others may adopt different (and non-economic) criteria but it is clear that, in adjudging the expertise of the Panel, then the expertise of Treasury officials is required to break the regress of ambiguous econometrics. In the case of putting all the faith of policy in one forecast,

this clearly presupposes some way of identifying the best one. Similarly, giving equal weighting to all the forecasts assumes some way of delineating the set of appropriate forecasts - boundary work in which the expertise of the Treasury Officials as economists in their own right is clearly important. Finally, in the third (and probably the most common) case, it is only Budd's own experience and competence as an economist which allows him to make these kinds of distinctions at all.

In conclusion, the role of the models in policy then seems to be rather slight. They may help economist to organise their thoughts and present consistent forecasts but in terms of actually testing economic theories or providing a 'scientific' basis for policy making, they seem to contribute very little:

> *Budd* One of the odd things about economic policy advice, among other things, is that people apparently give totally contradictory views year after year, which is very peculiar. You would think that life would select one of these and someone would be forced to admit that they were wrong. That, in itself is most peculiar ... What I have deduced [is] that if they did write down the conditions under which they would be wrong, then they would be conditions that would never occur ... They would regard an enormously wide range of possible outcomes as not refuting or in a weak sense consistent with what they are saying. So they are really making non-refutable statements, which to a Popperian suggests that they are not making scientific statements at all; they are making meta-physical statements.[60]

Thus, even within Her Majesty's Treasury, we find senior economists who are deeply sceptical of what economics models, as science, can achieve. Nowhere is it contested that economic models, and the forecasts they support, are invested with the skill and judgement of the economists which back them. Indeed, this scepticism is perhaps strongest amongst the economics community as it is here, as I have tried to show in this thesis, that economists are daily confronted with the gaps between the world and their models. Unfortunately, this point does not seem to be well known outside economic forecasting circles, although, as Alan Budd states, it is clearly important:

> *Budd* Although we think that Wynne Godley is very different from Patrick Minford, they are not really ... People should understand this. These guys who are being supported by one team or group or another, they are all saying the same thing ... They are really saying that the war will end this year, or maybe not. That is approximately what they are saying.[61]

Conclusions

Economic models appear to have no clear implication for policy at all. Because economic forecasters have yet to break out of the econometrician's regress and estimate an economic model that commands respect amongst economists, they have been unable to convince their own community of their theories. As a result, economists can only have a capricious and uncertain influence over economic policy. Although they can make coherent recommendations of their own, and their work can be used as a rhetorical resource in arguments for or against particular economic policies, the basis for these arguments does not rest on convincing empirical proofs. Rather, economic policies remains articles of faith rather than econometrically supported knowledge. As a result, economic theory and models function as a legitimisation (and quantification) of particular political and moral theories about the world.

This is an odd situation, and how to improve it is not obvious. For example, it is not clear whether we need less economists or more, but it is surely obvious that the nation would benefit from a better understanding of the basis for economic policy decision making. If the Panel of Economic Forecasters, by bringing a sense of the enormous range of economically legitimate views to the wider and public discussions, helps to foster an appreciation of both the uncertainty which surrounds economic policy, and the opportunities which exist for creating a different society, then this will be an important achievement.

The Panel of Forecasters and SSK

This final section reflects on the implications of the work for debates about symmetry and neutrality and considers the implications for this Sociology of a Social Science for SSK more generally. Thus far, the thesis has discussed a variety of issues arising from macro-economic modelling and forecasting. The aim has been to show how macro-econometric models are made and used. Throughout the story has been one of economic expertise being used to identify general patterns in historical and current economic data. The claim made has been that this is the most important part of the modelling and, particularly, the forecasting exercise.

Firstly, it is only by being socialised members of the economics community that economists can estimate econometric models in the first place. Secondly, it is only by being members of the wider community that economic forecasters have access to the nuances and knowledge which they need to use their models for forecasting. More

specifically, the expertise and skills of the forecasters are those of interpretation: not only interpreting the current data as an instance of a general case but also recognising the particular features of the current instance which will condition and mediate its outcome. Thus the economic forecasters are experts in the true sense. Like golfers who know how to adjust their swing to take account of the wind, the greens and the pin, so economic forecasters adjust their model to take account of the exogenous conditions.

This conclusion is no doubt appealing to economists, and is also adequate as SSK in that it outlines the ways in which economic models, forecasts and policy are constituted through social processes, institutions and interactions. However, it is only half the story. In addition to unpacking the econometric models and explicating the expertise of their proprietors, some less comfortable conclusions were also drawn (and not just by me). For example, throughout the thesis it has been apparent that economic models do not test economic theories and thereby provide a 'better' basis for economic policy making than anything else. In particular, it seems that neither econometric testing, nor economic data, distinguish between economic theories which have different moral and political implications for the society in which we live. In a way this might be a good thing - if economic science cannot make these choices, then some other way ought to be found. The problem at the moment, I would argue, is that this is not recognised and that, as a result, orthodoxy is uncritically re-presented as truth. Thus, although economic forecasters may be our best experts about the economic world, it is not clear how the econometric models through which their expertise is constituted actually help or benefit the wider community.

Symmetry and Neutrality

The sort of evaluative argument presented above is problematic from a SSK perspective (although, paradoxically, I think it would make sense to economists) as it seems to go against the tenets of symmetry and neutrality. As argued in Chapter 1 the study of economic modelling and forecasting raised particular problems for an SSK researcher, committed to Bloor's tenets, due to the ontological differences between their conception of the 'social' and that of the economic modellers.

In practice, however, the degree to which upholding the tenets of symmetry and neutrality is perceived or revealed as problematic depends on the level of analysis chosen. For example, in the chapters which discussed the Reports produced by the Panel of Independent Forecasters during 1993, the focus was primarily on the ways in

which data and evidence were interpreted by different economists. These chapters were thus most like 'normal' (in the Kuhnian sense) SSK, in that they were, essentially, documenting interpretive flexibility and analysing closure in scientific debate. Now, there is nothing wrong with this - economics, and economic forecasting, is an important science, the consequences of which affect the daily lives of every citizen. Thus, a sociological study which unpacks the way in which the 'facts' of economic life are constituted is an important and interesting task.

However, as was hinted at in the previous section, at another level of analysis, there are more serious problems. In particular, because of the different ontological commitments of the sociologist, much of what makes sense to an economist does not to the sociologist. In particular, the idea of trying to model social action mathematically, as a set of asocial equations, conflicts with sociology's emphasis on social collectivities. Thus, whilst the sociologist can be symmetrical about debates *within* economics, the situation is very different when it comes to debates *about* economics. In these situations, maintaining symmetry and neutrality is much more problematic. This is essentially the issue ducked by Ashmore *et al* in their study of health economics, in which they refuse to become embroiled in the problems of health care provision, preferring instead to remain curiously (un)applied analysts of expertise.

In this thesis I have tried to follow the example of Evelleen Richards who, in her study of the controversy over Vitamin C and Cancer, has combined sociological analysis with critique[62]. When focusing on the debate about Vitamin C, Richards is clearly symmetrical and neutral.[63] However, when the analysis shifts to questions about the ways in which Vitamin C, as a cancer therapy, was assessed then her commitment to symmetry and neutrality become more difficult. The reason is that, by virtue of the (symmetrical) sociological study, Richards does not believe that the randomised control trial was a very good way of asking the question in the first place.

In similar ways, symmetry and neutrality have never been far beneath the surface of the unpacking of economic modelling and forecasting presented in this thesis. The analysis has been at different levels in different chapters and the severity of the problems raised has varied accordingly. In Chapters 2 and 3, which dealt with the estimation and use of economic models for forecasting, the tension was probably the greatest as it was here that 'paradigm clash' was most apparent. In essence what the chapters present is a sociological deconstruction of an economic model. At one level

this analysis was symmetrical and neutral as *any* economic model could (and would) have been treated in the same way. However, on another level it was not symmetrical and neutral at all, as *all* economic models were implicated, yet the potential, reflexive deconstruction of my own economic model was not addressed.

In defence of this latter charge I would argue that the chapters represent a personal and 'ethnographic' account of how economic modelling is done. Part of any SSK methodology is socialisation into the scientific culture one wishes to write about. In my case this was economic modelling and coming to terms with regression analysis was part of that. The chapters re-present and document my surprise at what I found out when I began this process. Economic modelling was much more judgemental and contingent than I had expected. It turns out that this is a constitutive part of the subject and its debates. Indeed, without understanding how economic models work it is difficult to imagine how the controversies which characterise economics can have continued for so long. Thus, Chapters 2 and 3 are my attempt to convey the methods of econometrics.

However, there is more to economic forecasting than econometrics. Chapter 4 conveyed something of the difficulties of theory choice within economics. Here the tensions created by the requirements of symmetry and neutrality were less problematic. As an analyst of economics, rather than an economist, I have no interest in deciding which economist is the 'right' one and a symmetrical account was a relatively straightforward accomplishment. However, neutrality in this context was more difficult, though in an unexpected way. As a result of the symmetrical deconstruction of econometric models as a way of falsifying economic theories, I frequently find myself having to account for their existence in the first place! Thus, the argument that econometric models function as gatekeepers to the community of economic forecasters is my (sociological) defence of their existence.

Chapters 5, 6 and 7 are the least problematic in terms of symmetry and neutrality. Because the analysis is focused throughout on arguments within economics, symmetry and neutrality are again accomplished unproblematically. The same is true for the first part of this chapter, when the task is to examine the responses of the Panel to the events of 1993. This is, in effect, a re-run of the issues raised in Chapter 4, only with more empirical data as there is now a specific set of forecasts and outturns on which to focus. However, as soon as the focus moves away from economics (narrowly

conceived) to economic policy, symmetry and neutrality once again become problematic.

Unlike Chapters 2 and 3, where the conflict arose because of a tension between the different sorts of knowledge-claims made by sociologist and economists - a sort of academic turf-war - the difficulty in Chapter 8 occurs because as well as being a sociologist I am also a citizen. The tension here is over how economic policy ought to be made. Taken as a whole, the thesis clearly demonstrates that economic policy (and economic modelling and perhaps economic science as well) is, in several important ways, based on shared beliefs and convictions as much as empirical econometrics.[64] The question thus arises what are the implications of such an analysis for the making of economic policy?

Of course, one can, at this point do what Ashmore *et al* do and refuse to answer at all.[65] I do not find this a particularly satisfactory solution as I believe Ashmore *et al* miss the main point. The question is not about the actual allocation decisions made by health service managers, but how the decisions are made and the role of health economists in this process. Evelleen Richards sees this distinction and argues against Randomised Control Trials whilst remaining agnostic on whether or not Vitamin C 'cures' cancer.[66] Similarly, I do not want to legislate on economic policy, but do think that the research has implications for how economic policy is made.

In fact, and perhaps paradoxically, my sociologically informed conclusion is that the institutionalisation of the Panel of Forecasters has the potential to improve the *way* economic policy is made. Over the past two decades there has been a shift away from using economic models and forecasts to 'fine tune' economic policies in order to reach given policy targets. The economic climate is much less predictable and policy makers more cautious, with the emphasis now much more on avoiding mistakes. The advantage of the Panel in this context is that it has the potential to bring these uncertainties much more into the open. By keeping the openness of the economic future in the spotlight, the Panel can foster a climate in which it is respectable to admit that we really don't know what will happen next year. Indeed, one of the most important things highlighted by the Panel meetings is the different social, political and moral theories which are apparently compatible with decent economic growth. The Panel of Economic Forecasters thus offers moral choice instead of a narrowly (mis)represented economic orthodoxy and has the potential to return responsibility for social welfare back to the

political process. In other words, the Panel could re-politicise economic policy by legitimating the political debate of economic theories.

Conclusions

Returning to the problems of symmetry and neutrality in SSK, the study of macro-economic modelling and forecasting has shown than the accomplishment of these tenets is influenced by subject matter and analytical choices. By extending SSK to social rather than natural sciences, the taken-for-granteds of early SSK research are problematised. In particular, the notion that SSK has no relevance to the scientist at his or her laboratory bench raises two main issues when the scientists in question are social scientists making knowledge claims about the social world.

Firstly, there are the tensions raised when the SSK researcher and the scientists share the same subject matter. This conflict is most clearly apparent in the research over AI in which the subject matter - knowledge - is shared. However, the problem is not really 'knowledge', but how knowledge is understood differently by sociologists of science and AI researchers. Similarly, with economic modelling and forecasting, SSK is a social science and has its own understanding of how society can (and should) be understood. In the case of econometric modelling, this understanding is different from the conceptions and theories of the other scientists. I cannot legislate for others, but my own solution to this conundrum has not been to dismiss their work out of hand. Thus, as shown in Chapters 2 and 3 I have re-presented econometric practice in a sociologically consistent framework which shows how enculturation and shared beliefs provide the reference points which ground econometrics. Implicit in this is a critique of their method, but it is not something on which I have chosen to focus. The reasons are twofold. One is that economists are already pretty good at this sort of self-flagellation anyway. The other is that another, more interesting, issue is also raised.

This second issue relates to the use of economic models in the policy making process. Here the issues raised are much more important but also more difficult. The problem derives, in part, from the first one - that, as an SSK researcher, it is difficult to see what is gained as a result of this sort of asocial social science. However, the problem is made much more salient by virtue of the fact that the outputs and products of this science appear to be important in the making of economic policy. The issue is thus, if as an SSK researcher one comes to the conclusion that a particular social institution is not adequate for the task it is charged with, then should that point be made. I believe

that the answer is yes, but would qualify this by saying that it has to be for the 'right' reasons if SSK is to be a resource in that argument. More specifically, SSK can and should be used to critique and influence policy where the arguments and reasons for preferring one course of action over another stem from SSK itself. Thus, SSK cannot decide whether or not Vitamin C cures cancer or whether hysteresis exists in the labour market. However, it can ask the question 'Are the modes of knowledge production currently used to answer these questions appropriate?' In cases where the answer is 'No' then surely it is right to say so.

In conclusion, therefore, I would argue that this thesis has demonstrated the possibility of a more politically active form of SSK which takes as its subject matter those scientific processes within the policy making institutions. The aim of this sort of SSK is not to adjudicate in scientific disputes but to ask whether or not they are asking the right question. In the case of economics, I have demonstrated that economic forecasting is a more socially grounded and less mechanical exercise that I once thought. However, in terms of the policy debate, I remain concerned that it is artificially narrowed by the use of technical models which detach economic policy from socio-political responsibility.

Notes

[1] *Report of the Panel of Independent Forecasters, February 1994*, para 1.

[2] Source: op cit. note 1, para 1.

[3] op cit. note 1, para 2.

[4] op cit. note 1. para 3.

[5] Andrew Britton, *Submission to Report of Panel of Independent Forecasters, February 1994*, para 5

[6] Wynne Godley, *Submission to Report of the Panel of Independent Forecasters, February 1994,* para 17.

[7] Andrew Britton, op cit. note 5, paras 1&2.

[8] Andrew Britton, Interview, 1 March 1994, p. 6.

[9] David Currie, *Submission to Report of Panel of Independent Forecasters, February 1994,* para 26.

[10] David Currie, Interview, 24 February 1994.

[11] Wynne Godley, op cit. note 6, para 18.

[12] Wynne Godley, Interview, 9 June 1994, p. 2.

[13] Wynne Godley, op cit. note 12, p. 1.

[14] Wynne Godley, op cit. note 12, p. 1-2.

[15] Wynne Godley, op cit. note 11, para. 19.

[16] Wynne Godley, op cit. note 11, paras 20-23.

[17] Wynne Godley, op cit. note 12, pp. 2-3.

[18] Andrew Britton, op cit. note 5, para. 2.

[19] Andrew Britton, op cit. note 5, para. 4.

[20] David Currie, op cit. note 9, para. 27.

[21] Andrew Britton, op cit. note 5, para. 4.

[22] Andrew Britton, op cit. note 5, para. 21.

[23] Andrew Britton, op cit. note 5, para. 21.

[24] Andrew Britton, op cit. note 5, para 21.

[25] David Currie, op cit. note 9, para. 27.

[26] David Currie, op cit. note 9, para. 27.

[27] Andrew Britton, op cit. note 8, p. 4.

[28] Tim Congdon, *Submission to Report of the Panel of Independent Forecasters, February 1994*, paras. 21-22.

[29] Tim Congdon, op cit. note 28, para. 22.

[30] Tim Congdon, op cit. note 28, para. 23.

[31] Tim Congdon, op cit. note 28, para. 25.

[32] Tim Congdon, op cit. note 28, para. 25.

[33] Patrick Minford, *Submission to Report of Panel of Independent Forecasters, February 1994*, para. 22.

[34] Patrick Minford, op cit. note 33, para. 23.

[35] Patrick Minford, Interview, 11 March 1994, pp. 2-3.

[36] Patrick Minford, op cit. note 33, para. 24.

[37] Patrick Minford, op cit. note 35, p. 12.

[38] Patrick Minford, op cit. note 33, paras. 23-25.

[39] Patrick Minford, op cit. note 35, pp. 9-10

[40] Patrick Minford, op cit. note 35, pp. 12-13.

[41] Wynne Godley, *Submission to Report of the Panel of Independent Forecasters, July 1993*, para 7, fn 3

[42] Perhaps reflecting the mainstream view of the importance of monetary indicators.

[43] Gavyn Davies, Interview, 27 October 1993, p 13.

[44] Alan Budd, Interview 22 March 1994, p. 8.

[45] Alan Budd, op cit. note 44, p. 9.

[46] Andrew Britton, op cit. note 27, p. 12.

[47] Patrick Minford, op cit. note 35, p. 12.

[48] Tim Congdon, Interview, 2 March 1994, p. 17.

[49] Tim Congdon, op cit. note 48, pp. 18-19.

[50] Alan Budd, op cit. note 44, pp. 15.

[51] Tim Congdon, op cit. note 48, p. 18.

[52] Alan Budd, op cit. note 45, p. 15.

[53] Patrick Minford, op cit. note 35, pp. 16&17

[54] Patrick Minford, op cit. note 35, p. 15.

[55] Andrew Britton, op cit. note 27, p. 18.

[56] Alan Budd, op cit. note 44, p. 10.

[57] Alan Budd, op cit. note 44, p. 23.

[58] Alan Budd, op cit. note 44, p. 22.

[59] Alan Budd, op cit. note 44, pp. 22-23.

[60] Alan Budd, op cit. note 44, p. 25.

[61] Alan Budd, op cit. note 44, p. 26.

[62] Evelleen Richards (1991) *Vitamin C and Cancer: Medicine or Politics?* London: Macmillan; New York: St Martin's Press.

[63] Although Richards' book is not always read this way. See: Pinch, T. (1993) "Generations of SSK. Review of Richards, *Vitamin C and Cancer* and Sapp, *Where the Truth Lies*". *Social Studies of Science*, **23**, No. 2, pp. 363-73. For the reply, see: Evelleen Richards (1996) '(Un)Boxing the Monster' *Social Studies of Science*, **26**, No. 2, pp. 323-56

[64] Of course this is not necessarily a criticism of economic modelling, and to draw this conclusion would be to reinforce the very image of science which SSK originally set out to challenge (see Chapter 1). Rather, the key question is: how does re-conceptualising economic modelling in this way change the way we want to think about and make economic policy?

[65] Ashmore, M., Mulkay, M. and Pinch, T. (1989) *Health and Efficiency: A Sociology of Health Economics*. Milton Keynes: Open University Press.

[66] See Evelleen Richards, op cit. note 66

References

Bibliography

Arouh, A. (1978). *Empirical Testing and Theory Validation: A Structural Explanation of Persistent Controversy in Economics.* Unpublished PhD, University of Edinburgh.

Ashmore, M. (1993a). *Ending Up on the Wrong Side: Must the Two Forms of Radicalism Always be at War.* Paper presented at the Annual Meeting of the Society for the Social Studies of Science, November 1993, Purdue University, Indiana.

Ashmore, M. (1993b). 'The Theatre of the Blind: Starring a Promethean Prankster, a Phoney Phenomenon, a Prism, a Pocket, and a Piece of Wood'. *Social Studies of Science*, Vol. 23 (1), pp. 67-106.

Ashmore, M., Mulkay, M., & Pinch, T. (1989). *Health and Efficiency: A Sociology of Health Economics*. Milton Keynes: Open University Press.

Bank of England. (1993). *Bank of England Quarterly Bulletin.* (Vol. 33). London: Bank of England.

Barker, T. (1985). 'Forecasting the Economic Recession in the UK: A comparison based on ex ante forecasts'. *Journal of Forecasting*, Vol. 4, pp. 133-51.

Barnes, B., & Edge, D. (Eds.). (1982). *Science in Context: Readings in the Sociology of Science*. Milton Keynes: Open University Press.

Begg, D., Fisher, S., & Dornbusch, R. (1991). *Economics: 3rd Edition.* London: McGraw-Hill.

Bloor, D. (1973). 'Wittgenstein and Mannheim of the Sociology of Mathematics'. *Studies in the History and Philosophy of Science*, Vol. 4, pp. 173-91.

Bloor, D. (1976). *Knowledge and Social Imagery.* London: Routledge and Kegan Paul.

Bloor, D. (1983). *Wittgenstein: A Social Theory of Knowledge.* London: Macmillan.

Britton, A. (1993). 'Two Routes to Full Employment'. *National Institute Economic Review* (May 1993), pp. 5-11.

Britton, A. J. (Ed.) (1983). *The National Institute Model of the UK Economy*. London: Heinmann.

Britton, A. J., & Pain, N. (1992). *Economic Forecasting in Britain* (Report Series 4), National Institute of Economic and Social Research.

Cairncross, A. (1969) 'Economic Forecasting: Presidential Address the the Royal Economic Society, July 3, 1969', *The Economic Journal*, Vol. 79, pp. 797-812.

Callon, M. (1986a) 'The Sociology of an Actor-Network: The Case of the Electric Vehicle' in Callon, M., Law, J. and Rip, A. (Eds.) *Mapping the Dynamics of Science and Technology* (pp. 19-34). Basingstoke, UK: Macmillan

Callon, M. (1986b) 'Some Elements in the Sociology of Translation: Domestication of the Scallops and the Fishermen of St. Brieux Bay' in Law, J. (Ed.) *Power, Action Belief: A New Sociology of Knowledge?* (Sociological Review Monograph, pp. 196-229). London: Routledge and Kegan Paul.

Callon, M. and Latour, B. (1992) 'Don't Throw the Baby Out With the Bath School! A Reply to Collins and Yearley' in Pickering, A. (Ed.) *Science as Practice and Culture* (pp. 343-368). Chicago: University of Chicago Press.

Church, K. B., Mitchell, P. R., Smith, P. N., & Wallis, K. F. (1993). 'Comparative Properties of Models of the UK Economy'. *National Institute Economic Review* (August 1993), pp. 87-100.

Collins, H. M. (1974). 'The TEA Set: Tacit Knowledge and Scientific Networks'. *Science Studies*, Vol. 4, pp. 163-86.

Collins, H. M. (1975). 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics'. *Sociology*, Vol. 9, pp. 205-24.

Collins, H. M. (1985). *Changing Order: Replication and Induction in Scientific Practice*. London: Sage.

Collins, H. M. (1991). 'Captives and Victims: Response to Scott, Richards and Martin'. *Science Technology and Human Values*, Vol. 16, pp. 249-51.

Collins, H. M., & Pinch, T. J. (1993). *The Golem: What Everyone Needs to Know About Science*. Cambridge: Cambridge University Press.

Collins, H. M., & Pinch, T. J. (nd). *Unwanted Children: Essays in the Sociology of Fringe Science*: Unpublished manuscript.

Congdon, T. (1993a). 'A Critique of International Micawberism'. *Economic Affairs* (November/December 1993), pp. 13-18.

Congdon, T. (1993b) *Quarterly UK Economic Forecast, February 1993.* London: Lombard Street Research Ltd

Cyriax, G. (1981). *World Index of Economic Forecasts.* Farnborough, UK: Gower.

Davidson, C. (1994,). 'Heads for figures: Theoreticians are in great demand in the City'. *The Guardian,* 9 June, 1994.

Dewald, W. G., Thursby, J. G., & Anderson, R. G. (1986). 'Replication in Empirical Economics: The Journal of Money, Credit and Banking Project'. *American Economic Review,* Vol. 76 (4), pp. 587-603.

Evans, R. J. (1993a). *Economic Modelling: A Sociological Perspective.* Unpublished Dissertation for MSc in Social Research, University of Bath.

Evans, R. J. (1993b). *Soothsaying or Science: Falsification, Uncertainty and Social Change in Macro-econometric modelling.* Unpublished Dissertation for M.Sc in Social Research, University of Bath.

Evans, R. J. (1995). *How Do They Do That? The Role of Extrapolation, Econometrics and Judgement in the Economic Forecasts of the Seven Wise Men.* Paper presented at the Post Keynesian Study Group, 3 February 1995, University College London, London.

Friedman, M. (1953). The Methodology of Positive Economics. In M. Friedman (Ed.) *Essays in Positive Economics* (pp. 3-43). Chicago: Chicago University Press.

Friedman, M., & Schwartz, A. J. (1982). *Monetary Trends in the United States and the United Kingdom: Their Relation to Income, Prices and Interest Rates, 1867-1975.* Chicago: University of Chicago Press.

Garfinkel, H., Lynch, M. and Livingstone, E. (1981) 'The Work of Discovering Science Constructed with Material from the Optically Discovered Pulsar' *Philosophy of the Social Science,* Vol. 11, pp. 131-58

Godley, W. (1993). *Britain's Economic Prospects: No Recovery in Sight?* (The State of the Economy 1993, IEA Reading 39), Institute of Economic Affairs, London.

H.M. Treasury. (1993a). *Report of The Panel of Independent Forecasters, February 1993.* London: H.M. Treasury.

H.M. Treasury. (1993b). *Report of the Panel of Independent Forecasters, July 1993.* London: H.M. Treasury.

H.M. Treasury. (1993c). *Report of the Panel of Independent Forecasters, October, 1993.* London: H.M. Treasury.

H.M. Treasury. (1994). *Report of the Panel of Independent Forecasters, February 1994.* London: H.M. Treasury.

Hendry, D. F. (1986). 'The Role of Prediction in Evaluating Econometric Models'. *Proceedings of the Royal Society of London, Series "A" - Mathematical and Physical Sciences*, Vol. 407 (1832), pp. 25-34.

Hendry, D. F. (1989). *PC-Give: An Interactive Econometric Modelling System, version 6.0/6.01.*

Hendry, D. F., & Ericsson, N. R. (1987). *Assertion Without Empirical Basis: An Econometric Appraisal of Monetary Trends ..... In The United Kingdom by Milton Friedman and Anna J. Schwartz* (Applied Economics Discussion Paper 25), University of Oxford.

Holden, K. (1989). Macro-economic Models. In D. Greenaway (Ed.) *Current Issues in Macro-economics* . Basingstoke and London: Macmillan Education.

Holly, S. (1991). 'Economic Models and Economic Forecasting: Ptolemaic or Copernican?' *Economic Outlook* (June 1991), pp. 32-38.

Hudson, J., & Dymiotou-Jensen, M. (1989). *Modelling a Developing Country: A Case Study of Cyprus.* England: Avebury.

Jasanoff, S. (1992a) 'Science, Politics and the Renegotiation of Expertise and EPA' *Osiris*, Vol. 7, pp. 1-23

Jasanoff, S. (1992b) 'What Judges Should Know About the Sociology of Science' *Jurimetrics,* Vol. 32, pp. 343-359

Keating, G. (1985). *The Production and Use of Economic Forecasts.* London and New York: Methuen.

Keynes, J. M. (1936). *The General Theory of Employment, Interest and Money.* New York: Harcourt Brace.

Keynes, J. M. (1939). 'Professor Tinbergen's Method'. *Economic Journal*, Vol. 49, pp. 558-68.

Klein, L. R., Ball, R. J., Hazlewood, A., & Vandome, P. (1961). *An Econometric Model of the UK*. Oxford: Basil Blackwell.

Knorr-Cetina, K., & Mulkay, M. (Eds.) (1983). *Science Observed: Perspectives on the Social Study of Science*. London: Sage.

Kuhn, T. S. (1961). 'The function of measurement in modern physical science'. *Isis*, Vol. 52, pp. 162-76.

Kuhn, T. S. (1982). 'Normal Measurement and Reasonable Agreement'. In B. Barnes & D. Edge (Eds.), *Science in Context* . Milton Keynes: Open University Press.

Layard, R., Nickell, S., & Jackman, R. (1991). *Unemployment: Macroeconomic Performance and the Labour Market*. Oxford and New York: Oxford University Press.

Layard, R., Nickell, S., & Jackman, R. (1994). *The Unemployment Crisis*. Oxford: Oxford University Press.

Lucas, R. E. (1976). 'Econometric Policy Evaluation: A Critique' In Karl Brunner and Allan H. Meltzer (Eds.) *The Phillips Curve and Labour Markets*, Vol. 1 of the Carnegie-Rochester Conferences on Public Policy, a supplementary series to the *Journal of Monetary Economics* (North-Holland, Amsterdam), pp. 19-46

Lynch, M., Livingstone, E. and Garfinkel, H. (1983) 'Temporal Order in Laboratory Work' in Knorr-Cetina, K. and Mulkay, M. (Eds.) *Science Observed: Perspectives on the Social Study of Science*. London Sage.

MacKenzie, D.A. (1981) *Statistics in Britain: 1865-1930.* Edinburgh: Edinburgh University Press.

Marr, A. (1993). 'The Budget: Another Macleod on the horizon?' *Independent,* 1 December 1993, Budget page 13.

Martin, B. (1993). 'Prospects for the British Economy'. *Economic Affairs* (November/December 1993), pp. 5-12.

Mason, J., Mathias, P., & Westcott, J. H. (Eds.) (1986). *Predictability in Science and Society: A Joint Symposium of the Royal Society and The British Academy Held on 20 and 21 March, 1986*. London: Royal Society and British Academy.

McCloskey, D. N. (1986). *The Rhetoric of Economics*. Brighton: Wheatsheaf Books.

Minford, P. (1985). *Unemployment: Cause and Cure, 2nd ed.* Oxford, UK ; New York, NY: Basil Blackwell.

Mulkay, M., & Gilbert, N. (1984). *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse.* Cambridge: Cambridge University Press.

NIESR. (1988). 'The UK Economy'. *National Institute Economic Review*, Vol. No. 123 (February 1988).

Oreskes, N., Shrader-Frechette, K., & Belitz, K. (1994). 'Verification, Validation and Confirmation of Numerical Models in the Earth Sciences'. *Science*, Vol. 263 (4 February 1994), pp. 641-46.

Ormerod, P. (1992a). 'Crisis in the Exchange Rate'. *Times Higher Education Supplement*, 18 September 1992, pp. 18 & 17.

Ormerod, P. (1992b). *On the Dynamics of Capitalism.* Paper presented at the Paper presented to the Macro and Money Study Group Conference, September 1992, Imperial College, London.

Pain, N., & Britton, A. J. (1992). 'National Institute Economic Forecasts 1968 to 1991: Some tests of forecast properties'. *National Institute Economic Review.*, Vol. 141 (August 1992), pp. 81-93.

Parkin, M., & King, D. (1992). *Economics.* Wokingham, England: Addison-Wsley Publishing.

Pennington. (1993). 'Pleasing Gamble Might Come Off'. *The Times,* 1 December 1993, p. 29.

Pickering, A. (Ed.) (1992). *Science as Practice and Culture.* Chicago and London: University of Chicago Press.

Pinch, T. (1993). 'Generations of SSK. Review of Richards, Vitamin C and Cancer and Sapp, Where the Truth Lies'. *Social Studies of Science*, Vol. 23 (2), pp. 363-73.

Pinch, T., & Ashmore, M.. (1992). *Defending the Daft and the Dangerous: Wilheim Reich and the strange case of Orgone Energy.* Paper presented at the Annual Meeting of the Society for Social Studies of Science, 13 August 1992, Gothenburg, Sweden.

Pindyck, R. S., & Rubinfeld, D. L. (1991). *Econometric Models and Economic Models: 3rd edition.* New York: McGraw-Hill International Editions.

Porter, T. (1995). *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life*. Princeton, N.J.: Princeton University Press.

Ramanathan, R. (1992). *Introductory Econometrics; with applications. 2nd Edition.* Florida, USA: Harcourt Brace Jovanovich International.

Richards, E. (1991). *Vitamin C and Cancer: Medicine or Politics*? London: Macmillan.

Richards, E. (1996). '(Un)Boxing the Monster'. *Social Studies of Science*, Vol. 26 (2), pp. 323-56.

Scott, P., Richards, E., & Martin, B. (1990). 'Captives of Controversy: The Myth of the Neutral Social Researcher in Contemporary Scientific Controversies'. *Science Technology and Human Values*, Vol. 15, pp. 474-94.

Scott, P., Richards, E., & Martin, B. (1991). 'Whose a Captive? Whose a Victim? Response to Collins's Method Talk'. *Science Technology and Human Values*, Vol. 16, pp. 252-5.

Sentance, A. (1992). 'Rebalancing the British Economy'. *The Business Economist*, Vol. 24 (1 (Winter 1992)), pp. 22-32.

Shackley, S. and Wynne, B. (1996) 'Representing Uncertainty in Global Climate Change Science and Policy: Boundary Ordering Devices and Authority' *Science Technology and Human Values*, Vol. 21 (3), pp. 275-302.

Shapin, S., & Schaffer, S. (1985). *Leviathan and the Air-Pump : Hobbes, Boyle and the Experimental Life*. Princeton, N.J.: Princeton University Press.

Surrey, M. J. C. (Ed.) (1971). *The Analysis and Forecasting of the British Economy.* London and New York.: Methuen.

Turner, D. S. (1990). 'The Role of Judgement in Economic Forecasting'. *Journal of Forecasting*, Vol. 9, pp. 315-45.

Vaughan, L. (1993,). 'The Budget: "Wise Men" advise interest rate cut: Economic advisers' verdicts'. *The Independent,* 1 December 1993, Budget Page 12.

Wallace, T. D., & Silver, J. L. (1988). *Econometrics: An Introduction.* Massachusetts, USA: Addison Wesley Publishing.

Wallis, K. F., & Whitely, J. (1991). 'Sources of Error in Forecasts and Expectations: UK Economic Models, 1984-8'. *Journal of Forecasting*, Vol. 10, pp. 231-253.

Wallis, K. F. (Ed.), Andrews, M. J., Fisher, P. G., Longbottom, J. A., & Whitely, J. D. (1987). *Models of the UK Economy: Third review by the ESRC Macromodelling Bureau.* Oxford: Oxford University Press.

Wallis, K. F. (Ed.), Andrews, M. J., Fisher, P. G., & Whitley, J. D. (1984). *Models of the UK Economy: A Review by the ESRC Macromodelling Bureau.* Oxford: Oxford University Press.

Winch, P. (1958/1988). *The Idea of a Social Science and its Relationship to Philosophy.* London and New York: Routledge.

Wren-Lewis, S. (1992). *Macroeconomic Theory and UK Macroeconomic Models: Another Failed Partnership?* (Discussion paper 9), International Centre for Macroeconomcic Modelling, University of Strathclyde.

Wynne, B. (1989) 'Sheep Farming After Chernobyl: A Case Study in Communicating Scientific Information' *Environment Magazine*, Vols. 31 and 32, pp. 10-15, 33-39

Wynne, B. (1992) 'Uncertainty and Environmental Learning: Reconceiving Science and Policy in the Preventative Paradigm' *Global Environmental Change*, Vol. 2, pp. 137-154.

Interviews

Feb1993

Name	Date
John Hudson	15.01.93
Paul Ormerod	16.03.93
Ken Wallis	21.03.93
Patrick Minford	31.03.93
Wynne Godley	05.04.93
Andrew Britton	29.04.93
David Currie	13.05.93

July1993

Patrick Minford	??.07.93
Andrew Sentance	13.07.93
Tim Congdon	13.07.93
David Currie	13.07.93
Andrew Britton	12.07.93

Oct1993

Andrew Britton	27.10.93
Gavyn Davies	27.10.93
Andrew Sentance	29.10.93
Tim Congdon	29.10.93

Feb1994

David Currie	24.02.94
Andrew Britton	01.03.94
Tim Congdon	02.03.94
Patrick Minford	11.03.94
Alan Budd	22.03.94

June1994

Tim Congdon	02.06.94
Patrick Minford	03.06.94
Andrew Britton	06.06.94
Wynne Godley	09.06.94

Appendix A

Listing of equations used in econometric model. Numbers in parentheses and beneath each coefficient are the *t* statistic for each coefficient.

Key

G	General Government Final Consumption plus Total Public Sector Fixed investment
GDPE	Gross Domestic Product, Expenditure Estimate
C	Consumers Expenditure
X	Non-Oil Exports
M	Non-Oil Imports
O	Oil Exports net of Oil Imports
KII	Stock of Inventories
KI	Stock of Fixed Capital of the Private Sector
PC	Consumer Prices Index
YD	Personal Disposable Income
RLB	UK Bank's Base Rates
EER	Sterling's Effective (trade weighted) Exchange Rate
ET	Employed Labour Force
WSI	Index of Average Wages and Salaries
RSW	World Short Term Interest Rates
WWPI	World Wholesale Price Index
XWM	Index of World Exports of Manufactures
POILWC	Index of Oil Prices in 'World' Currency
WGNP	Index of World GNP
M0	Money Supply, M0, not seasonally adjusted
PSBR	Public Sector Borrowing Requirement
DRESV	Change in Reserves
GOTVB	Stock of Non-Money Government Debt
BAL	Current Account Balance
OJ	Private Sector Net Overseas Financial Assets
XEER	Expectations of the Exchange Rate one period ahead
COMP	Index of Competitiveness (i.e. Real Exchange Rate)

PX	Non-Oil Export Price Index
T	Average Income Tax Rate
TREF	Average Indirect Tax rate
TIME	Time Trend
ICHS	Sales of Council Houses

Equations

Prices

$$PROD = 1.0057^{(TIME\ -32)}$$

$$PC_t = \exp[\ \log(PC_{t-1}) - \underset{(-0.19)}{0.027}(PC_{t-1}/PC_{t-2}) + \underset{(4.63)}{0.125}\log(TREF_t) - \underset{(-2.68)}{0.090}\log(TREF_{t-1}) - \underset{(-2.10)}{0.049}\log(TREF_{t-4}) + \underset{(0.08)}{0.001}\log(PC_{t-1}/(WWPI_{t-1}/EER_{t-1})) + \underset{(1.68)}{0.043}\log(GDPE_{t-1}/PROD) - \underset{(-1.52)}{0.467}]$$

Estimation period from 1981Q2 to 1992Q4
R bar^2 = 0.40;
std. error of estimate = 0.005;
Durbin-Watson = 2.32

Exchange Rate

$$EER_t = XEER_t(1 + (RLB_t - RSW_t)/400)$$

Wages

$$WSI_t = \exp[\ \log(WSI_{t-1}) + \log(WSI_{t-1}/WSI_{t-2}) + \underset{(0.22)}{0.033}\ (\log(WSI_{t-2}/WSI_{t-3}) - \log(WSI_{t-3}/WSI_{t-4})) - \underset{(-5.05)}{1.070}\ (\log(WSI_{t-1}/(1.0057 \times PC_{t-1})) - (\log(WSI_{t-1}/PC_{t-1})) - \underset{(-0.60)}{0.561}\ (\log(PC_{t-1}/PC_{t-2}) - \log(PC_{t-2}/PC_{t-3})) - \underset{(-1.09)}{0.010}$$

Estimation period from 1980Q4 to 1992Q4
R bar^2 = 0.49;
std. error of estimate = 0.168;
Durbin-Watson = 2.02

Competitiveness

$$COMP_t = WSI_t/(PROD \times WWPI_t/EER_t)$$

Export Demand

$$\log(X_t) = \exp[\underset{(5.02)}{0.800}\log(X_{t-1}) + \underset{(0.81)}{0.125}\log(X_{t-2}) + \underset{(0.74)}{0.062}\log(XWM_t) - \underset{(-0.51)}{0.023}\log(COMP_t) + \underset{(0.95)}{0.618}$$

Estimation period from 1980Q4 to 1992Q4
R bar^2 = 0.98;
std. error of estimate = 0.025;
Durbin-Watson = 1.86

Export Prices

$$\log(PX_t) = \exp[\underset{(10.71)}{0.857}\log(PX_{t-1}) - \underset{(-2.28)}{0.064}\log(WSI_{t-1}/PROD_t) + \underset{(1.49)}{0.054}\log(WWPI_{t-1}/EER_{t-1}) + \underset{(2.55)}{0.918}$$

Estimation period from 1981Q1 to 1992Q4
R bar^2 = 0.97;
std. error of estimate = 0.019;
Durbin-Watson = 1.64

Fixed Investment

$$KI_t = KI_{t-1} + GDPE_{t-1}\exp[\underset{(13.99)}{0.923}\log((KI_{t-1} - KI_{t-2})/GDPE_{t-2})) + \underset{(0.44)}{0.040}\log(KI_{t-1}/GDPE_{t-1}) + \underset{(1.63)}{1.857}\log(GDPE_{t-1}/GDPE_{t-2}) + \underset{(0.38)}{0.042}\log((WSI_{t-1} \times ET_{t-1})/(PC_{t-1} \times 1.0057^{TIME})) - \underset{(-0.52)}{0.617}$$

Estimation period from 1980Q3 to 1992Q4
R bar^2 = 0.90;
std. error of estimate = 0.062;
Durbin-Watson = 2.29

Flow of Gross Private Fixed Investment

$$IFP_t = KI_t - KI_{t-1} + 4610^{(TIME - 100)}$$

Inventories

Expected Real Rate of Interest

$$RREAL_t = 0.871\ RLB_{t-1} - 0.383\ (400(EER_{t-1}/(\ EER_{t-1} - 1)))$$
$$\qquad\qquad (9.16) \qquad (0.81)$$
$$+ 0.005\ (400(((WWPI_{t-1} \times EER_{t-2})/(WWPI_{t-2} \times EER_{t-1})) - 1)$$
$$\quad (0.40)$$
$$- 0.003\ (400(WSI_{t-1}/WSI_{t-2} - 1) + 157.180$$
$$\quad (-0.28) \qquad\qquad\qquad (0.82)$$

Estimation period from 1981Q2 to 1992Q4
R bar^2 = 0.67;
std. error of estimate = 1.212;
Durbin-Watson = 1.71

$$KII_t = \exp[\ \log(KII_{t-1} - 0.00002\ RREAL_t + 0.534\ \log\ (KII_{t-1}/KII_{t-2})$$
$$\qquad\qquad\qquad (-0.60) \qquad\qquad (4.37)$$
$$+ 0.00003\ \log(KII_{t-1}/GDPE_{t-1}) - 0.0002$$
$$\quad (0.08) \qquad\qquad\qquad (.06)$$

Estimation period from 1981Q2 to 1992Q4
R bar^2 = 0.27;
std. error of estimate = 0.0004;
Durbin-Watson = 2.13

Employment (whole economy)

$$ET_t = \exp[\log(ET_{t-1}) + 0.310\ \log(ET_{t-1}/ET_{t-2}) - 0.129\ \log(ET_{t-1} \times PROD/GDPE_{t-1})$$
$$\qquad\qquad\qquad (2.07) \qquad\qquad\qquad (-3.71)$$
$$- 0.0005\ \log\ (TREF_{t-1}/PC_{t-1}) - 0.0002\ \log(WSI_{t-1}/(PROD \times PC_{t-1}))$$
$$\quad (-0.44) \qquad\qquad\qquad (-0.06)$$
$$- 0.178$$
$$\quad (-3.55)$$

Estimation period from 1984Q1 to 1992Q4
R bar^2 = 0.69;
std. error of estimate = 0.029;
Durbin-Watson = 1.75

Consumer Spending

$$C_t = \exp[\log(C_{t-1}) + 0.0057 - 0.125\,(\log(C_{t-1}/C_{t-2}) - 0.0057)$$
$$\qquad\qquad\qquad\qquad\qquad\quad (-1.45)$$
$$- 0.026 \log(C_{t-1}/(YD_{t-1}/(PC_{t-1}/100))) - 0.197\,(\log(PC_{t-1}/PC_{t-2}) - 0/0057)$$
$$\quad (0.93) \qquad\qquad\qquad\qquad\qquad (-1.96)$$
$$+ 0.228\,(\log((YD_{t-1}/PC_{t-1})/(YD_{t-2}/PC_{t-2})) - 0.0057)$$
$$\quad (3.17)$$
$$- 0.0009\,(RLB_{t-1} - RLB_{t-2})$$
$$\quad (-1.04)$$

Estimation period from 1963Q3 to 1992Q4
$R\ \text{bar}^2 = 0.15$
std. error of estimate = 0.012;
Durbin-Watson = 2.23

Imports (non-oil)

$$M_t = \exp[\,0.805 \log(M_{t-1}) + 0.791 \log(C_t/C_{t-1})$$
$$\quad (11.34) \qquad\qquad (2.87)$$
$$+ 20.136 \log((KII_t/KII_{t-1})/(KII_{t-1}/KII_{t-2}))$$
$$\quad (2.30)$$
$$+ 0.370 \log(GDPE_{t-2}) - 2.337$$
$$\quad (2.71) \qquad\qquad (-2.61)$$

Estimation period from 1970Q2 to 1992Q4
$R\ \text{bar}^2 = 0.98$
std. error of estimate = 0.033;
Durbin-Watson = 2.08

Expenditure Measure of GDP

Adjustment to Factor Cost

$$F = 0.139\,C_t + 0.053\,IFP_t - 0.005\,X_t + 0.143\,G_t$$

$$GDPE = C_t + IFP_t + KII_t - KII_{t-1} + G_t + X_t - M_t + O_t - F_t$$

Financing the PSBR

Government Debt

$$DGG_t = PSBR_t - (MO_t - MO_{t-1}) + DRESV_t$$

Stock of Government Debt

$$GOVTB_t = GOTVB_{t-1} + DGG$$

Appendix B

Data sources and references for econometric model

```
          ESRC CENTRE IN ECONOMIC COMPUTING

    TIME SERIES DATA BANK - RELEASE DATE        94

=====================================================================
=== List of publications                                          ===
 BB    BB
 CI    CI
 DDIM  DDIM
 EG    EG
 EGU   EGU
 ET    Economic Trends
 ETAS  ETAS
 FSC   FSC
 FSF   FSF
 GDPM  GDPM
 GDPO  GDPO
 IOP   IOP
 MD    Monthly Digest of Statistics
 PB    PB
 PPI   PPI
 PSBR  PSBR
 QA    QA
 RPI   RPI
=====================================================================


         ***  DATA RETRIEVAL  ***

Periodicity :  Quarterly

Sample period    46  1 TO   94  4      No. observations    196

No. series retrieved    19

                                                                  Start Base
                                                                  YYPPP YYMM

 TABLE QA   A06    - General Govt: GDFCF: CP NSA: BPVDL: D0-9
 -----------------
DFDA DFDA Public Corps: GDFCF: K90 SA: BPVDL: D0-9                62  1 90 0
          Seasonally adjusted
                                              SIC : D0-9

 TABLE ET     2.2  - Consumers' expenditure : Total #m CONS (1990 prices) SA
 -----------------
CAOO CAOO GDP(A) at constant market prices     (1990 prices)      55  1 90 0
          Seasonally adjusted
CAAB CAAB Consumers' expenditure : Total #m CONS (1990 prices) SA  55  1 90 0
          Seasonally adjusted

 TABLE QA   A02    - General govt : taxes on expenditure
 -----------------
DJDG DJDG Goods and services: total exports (credits), CONSTANT PR 55  1 90 0
          Seasonally adjusted

 TABLE MD    15.1  - Food beverages and tobacco: OTS : Exports by commodity
 -----------------
BOCD BOCD Fuels : OTS : Exports by commodity                       70  1  0 0
          Seasonally adjusted

 TABLE MD     1.2  - General govt : taxes on expenditure
 -----------------
DJDJ DJDJ Goods and services: total imports (debits), CONSTANT PRI 55  1 90 0
          Seasonally adjusted

 TABLE MD    15.1  - Food beverages and tobacco: OTS : Exports by commodity
 -----------------
BODD BODD Imports Fuels : OTS                                     70  1  0 0
          Seasonally adjusted
```

```
 TABLE QA   X502   - COS: ICCs: TOTAL CAPITAL TRANSFERS
 -----------------
FMCD FMCD COS: Increase in stocks and work in progress #m NSA*     66  1   0 0

 TABLE MD     1.8  - Public Corps: GDFCF: CP NSA: BPVDL: D0-9
 -----------------
DFEB DFEB Private Sector: GDFCF: K90 SA: BPVDL: D0-9               62  1 90 0
          Seasonally adjusted
                                                SIC : D0-9

 TABLE FSC    1    - Central govt : current surplus or deficit
 -----------------
AIIX AIIX Consumers' expenditure: Total #m CURR SA                 55  1   0 0
          Seasonally adjusted

 TABLE ET     6.1  - Official reserves outstanding : total US$M
 -----------------
AJHV AJHV Sterling effective exchange rate index 1985=100          75  1 85 0

 TABLE MD     3.1  - Employees in employment (UK) - thousands (EG table 1.1)
 -----------------
BCAJ BCAJ Employees in employment (UK) - thousands (EG table 1.1)  59  2   0 0
          Seasonally adjusted

 TABLE ETAS   3.1  - Wages & salaries per unit of output index : whole economy
 -----------------
DNAA DNAA Average earnings (GB) index : whole economy (1990=100)   88  1 90 0
                                              SIC : 0-9

 TABLE FSF    1    - Central govt : current surplus or deficit
 -----------------
AJIB AJIB Interest on US dollar deposits in London (3 month)       63  1   0 0

 TABLE FSC    1    - Central govt : current surplus or deficit
 -----------------
AAGE AAGE Public sector finance : notes & coin #m                  63  1   0 0

 TABLE ET     6.5  - General govt : financial surplus or deficit
 -----------------
ABFB ABFB Public sector borrowing requirement (PSBR) #m (CYSA)     63  1   0 0
          Seasonally adjusted

 TABLE FSF    1    - Central govt : current surplus or deficit
 -----------------
AACM AACM CG    : liabs: flows: Other government overseas financin 63  2   0 0
AIPA AIPA Central govt finance: official reserves #m               46  1   0 0

 TABLE FSC    1    - Central govt : current surplus or deficit
 -----------------
AAAA AAAA Central govt : current surplus or deficit                55  1   0 0


 TABLE ETAS   1.3  - General govt : taxes on expenditure
 -----------------
AAXP AAXP General govt : taxes on expenditure - CYSA               55  1   0 0
          Seasonally adjusted

 TABLE MD     1.2  - General govt : taxes on expenditure
 -----------------
DJDJ DJDJ Goods and services: total imports (debits), CONSTANT PRI 55  1 90 0
          Seasonally adjusted

 TABLE MD     1.5  - Personal sector: saving #m
 -----------------
AIIW AIIW Personal disposable income #m                            55  1   0 0
          Seasonally adjusted

 TABLE MD     3.1  - Employees in employment (UK) - thousands (EG table 1.1)
 -----------------
BCAJ BCAJ Employees in employment (UK) - thousands (EG table 1.1)  59  2   0 0
          Seasonally adjusted

 TABLE FSF    1    - Central govt : current surplus or deficit
 -----------------
AJIB AJIB Interest on US dollar deposits in London (3 month)       63  1   0 0
```

```
 TABLE FSF    1     - Central govt : current surplus or deficit
 -----------------
AACM AACM CG     : liabs: flows: Other government overseas financin 63  2  0 0
AIPA AIPA Central govt finance: official reserves #m                46  1  0 0


 TABLE MD    15.1  - Food beverages and tobacco: OTS : Exports by commodity
 -----------------
BOCD BOCD Fuels : OTS : Exports by commodity                        70  1  0 0
          Seasonally adjusted

 TABLE QA   A02    - General govt : taxes on expenditure
 -----------------
DJAZ DJAZ Goods and services: total exports (credits), CURRENT PRI 55  1  0 0
          Seasonally adjusted

 TABLE QA   A09    - Personal sector: total personal income #m
 -----------------
AIIU AIIU Personal sector: UK taxes on income #m                    55  1  0 0
          Seasonally adjusted

 TABLE ETAS   1.6  - Personal sector: total personal income #m
 -----------------
AIIQ AIIQ Personal sector: total personal income #m                 55  1  0 0
          Seasonally adjusted

 TABLE MD     1.2  - General govt : taxes on expenditure
 -----------------
AAXW AAXW General govt : subsidies - CYSA                           55  1  0 0
          Seasonally adjusted

 TABLE ETAS   1.3  - General govt : taxes on expenditure
 -----------------
AAXP AAXP General govt : taxes on expenditure - CYSA                55  1  0 0
          Seasonally adjusted

 TABLE ETAS   1.2  - Invisibles (balance) : interest , profits & dividends #m
 -----------------
DIAS DIAS General govt : adjustment to factor cost @ 1990 prices - 55  1 85 0
          Seasonally adjusted


 TABLE BB    14.6  - Personal sector: NDFCF: dwellings #m  (Annual)
 -----------------
EXGB EXGB Personal sector: NDFCF: all fixed assets #m               48  1  0 0
EXGC EXGC I&C companies: NDFCF: all fixed assets #m                 48  1  0 0
EXGD EXGD Financial companies: NDFCF: other fixed assets #m         48  1  0 0
```

Appendix C

Program Code for econometric model.

Software used was RATS 4.0; Filename = "MODEL2.PRG".

```
CALENDAR 1955 1 4
ALLOCATE 1994:4
OPEN DATA C:\ROBERT\MACROMOD\RATFILE1.WK3
DATA(FORMAT=WKS, ORG=OBS) 1955:1 1992:4 G GDPE C X M O KII KI PC $
 YD RLB EER ET WSI RSW WWPI XWM POILWC WGNP MONEY PSBR DRESV GOVTB $
 BAL OJ XEER COMP PX TAX TREF TIME FACTOR

TABLES

SET PROD = 1.0057**(TIME - 32)
*
*
*
SEASONAL Q1 1955:1 1994:4 4 1955:1
SEASONAL Q2 1955:1 1994:4 4 1955:2
SEASONAL Q3 1955:1 1994:4 4 1955:3
*
*
*
* PRICES

SET LOGPC = LOG(PC)
SET PCRATIO = LOGPC - LOGPC{1}
SET LOGTREF = LOG(TREF)
SET PCVAR6 = LOGPC{1} - LOG(WWPI{1}/EER{1})
SET PCVAR7 = LOG(GDPE{1}/PROD)

SET PC = EXP(PCRATIO + LOGPC{1})

* INTEREST RATES

SET MOPCRAT = LOG(MONEY/PC{1})
SET GDPEVAR = LOG(GDPE - O)
SET TIMELAG = TIME - 32

* WAGES

SET LOGWSI = LOG(WSI)
SET WSIRATIO = LOG(WSI/WSI{1})
SET WAGEVAR = WSIRATIO - WSIRATIO{1}
SET WAGEVAR2 = LOG(WSI/(1.0057*PC)) - LOG(WSI{1}/PC{1})
SET WAGEVAR3 = PCRATIO - PCRATIO{1}

SET WSI = EXP( LOGWSI{1} - 0.009703990 $
  + 0.03276270*WAGEVAR{1} - 1.070075715*WAGEVAR2{1} - 0.561039629*WAGEVAR3{1})

* EXPORT DEMAND

SET LOGX = LOG(X)
SET LOGXWM = LOG(XWM)
SET LOGCOMP = LOG(COMP)

SET X = EXP(LOGX)

* EXPORT PRICES

SET LOGPX = LOG(PX)
SET WSIVAR = LOG(WSI{1}/PROD)
SET WWPIVAR = LOG(WWPI/EER)

SET PX = EXP(LOGPX)

* FIXED INVESTMENT

SET KIVAR1 = LOG((KI - KI{1})/GDPE{1})
SET KIVAR2 = LOG(KI/GDPE)
```

```
SET KIVAR3 = LOG(GDPE/GDPE{1})
SET KIVAR4 = LOG((WSI{1}*ET{1})/(PC{1}*(1.0057**TIME)))

SET KIFITTED = KI{1} + GDPE{1}*EXP(-0.616613709 $
  + 0.923349555*KIVAR1{1} + 0.035927732*KIVAR2{1} + 1.857239412*KIVAR3{1} $
  + 0.042422053*KIVAR4)

SET KI = KIFITTED

* VARIABLE FOR FLOW OF GROSS FIXED PRIVATE INVESTMENT

SET IFP = KIFITTED - KIFITTED{1} + 4610*1.0086**(TIME-100)

* INVENTORIES

SET RLBVAR1 = 400*(EER/(EER - 1))
SET RLBVAR2 = 400*((WWPI*EER{1})/(WWPI{1}*EER) - 1)
SET RLBVAR3 = 400*(WSI/(WSI{1} - 1))

SET RREAL = 157.1804378 + 0.8709958*RLB{1} - 0.3832469*RLBVAR1{1} $
  + 0.0047546*RLBVAR2{1} - 0.0025148*RLBVAR3{1}

SET LOGKII = LOG(KII)
SET KIIRATIO = LOG(KII/KII{1})
SET KIIGDPE = LOG(KII/GDPE)

SET KII = EXP(KIIRATIO + LOGKII{1})

* EMPLOYMENT

SET LOGET = LOG(ET)
SET ETDIFF = LOG(ET/ET{1})
SET ETVAR2 = LOG(ET{1}*PROD/GDPE{1})
SET ETVAR3 = LOG(TREF/PC)
SET ETVAR4 = LOG(WSI{1}/(PROD*PC{1}))

SET ET = EXP(ETDIFF + LOGET{1})

* CONSUMER SPENDING

SET LOGC = LOG(C)
SET CDIFF = LOG((C/C{1}) - 0.0057)
SET CVAR2 = LOG(C/(YD/(PC/100)))
SET CVAR3 = LOG((PC/PC{1}) + 0.0057)
SET CVAR4 = LOG((YD/PC)/(YD{1}/PC{1}))
SET RLBDIFF = RLB - RLB{1}

SET C = EXP(LOGC{1} + 0.0057 - 0.125784814*CDIFF{1} $
  - 0.026344580*CVAR2{1} - 0.197419823*CVAR3{1} $
  + 0.228154705*CVAR4{1} + 0.0009558958*RLBDIFF{1})

* NON-OIL IMPORTS

SET LOGM = LOG(M)
SET CDIFFM = LOG(C/C{1})
SET MVAR3 = LOG((KII/KII{1})/(KII{1}/KII{2}))
SET LOGGDPE = LOG(GDPE)

SET M = EXP(LOGM)

* ADJUSTMENT TO FACTOR COST

SET FACTOR = 0.139228970*C + 0.053169319*IFP -  0.005139745*X $
  + 0.143336863*G

* VARIABLE FOR GILTS AND OTHER GOVT. DEBT

SET DGG = PSBR -(MONEY-MONEY{1}) + DRESV

* EQUATION FOR PRICES

EQUATION PCRATIOEQ PCRATIO
# CONSTANT PCRATIO{1} LOGTREF LOGTREF{1} LOGTREF{4} PCVAR6{1} PCVAR7

ASSOCIATE PCRATIOEQ
# -0.467475118 -0.027154131 0.124679170 -0.089689457 -0.049091458 $
  0.001167316 0.042513803

FRML(EQUATION=PCRATIOEQ) PCRATIOFM
```

```
* EQUATION FOR INTEREST RATES
* NOW MADE AS EXOGENOUS ASSUMPTION

*EQUATION RLBEQ RLB
*# CONSTANT MOPCRAT MOPCRAT{1} GDPEVAR{1} TIMELAG Q1 Q2 Q3

*ASSOCIATE RLBEQ
*# -419.90231 -338.83373 269.23895 74.101853 0.6357537 $
*  -25.338732 -22.119168 -18.822553

*FRML(EQUATION=RLBEQ) RLBFM

* EQUATION FOR WAGES

EQUATION WAGESEQ WAGEVAR
# CONSTANT WAGEVAR{1} WAGEVAR2{1} WAGEVAR3{1}

ASSOCIATE WAGESEQ
# -0.009703990 0.03276270 -1.070075715 -0.561039629

FRML(EQUATION=WAGESEQ) WAGESFM

* EQUATION FOR EXPORT DEMAND

EQUATION LOGXEQ LOGX
# CONSTANT LOGX{1 2} LOGXWM LOGCOMP

ASSOCIATE LOGXEQ
# 0.617887884 0.795274905 0.125478551 0.061604870 -0.023141413

FRML(EQUATION=LOGXEQ) LOGXFM

* EQUATION FOR EXPORT PRICES

EQUATION LOGPXEQ LOGPX
# CONSTANT LOGPX{1} WSIVAR WWPIVAR{1}

ASSOCIATE LOGPXEQ
# 0.918356387 0.857347125 -0.063864886 0.053945906

FRML(EQUATION=LOGPXEQ) LOGPXFM

* EQUATION FOR FIXED INVESTMENT

EQUATION KIEQ KIVAR1
# CONSTANT KIVAR1{1} KIVAR2{1} KIVAR3{1} KIVAR4

ASSOCIATE KIEQ
# -0.616613709 0.923349555 0.035927732 1.857239412 0.042422053

FRML(EQUATION=KIEQ) KIFM

* EQUATION FOR INVENTORIES

EQUATION KIIEQ KIIRATIO
# CONSTANT RREAL KIIRATIO{1} KIIGDPE{1}

ASSOCIATE KIIEQ
# 0.000093070 -0.000017713 0.533718439 0.000041711

FRML(EQUATION=KIIEQ) KIIFM

* EQUATION FOR EMPLOYMENT(WHOLE ECONOMY)

EQUATION ETEQ ETDIFF
# CONSTANT ETDIFF{1} ETVAR2 ETVAR3 ETVAR4

ASSOCIATE ETEQ
# -0.178353081 0.310422237 -0.128975991 -0.000454412 -0.000241498

FRML(EQUATION=ETEQ) ETFM

* EQUATION FOR CONSUMER SPENDING

EQUATION CEQ CDIFF
# CDIFF{1} CVAR2{1} CVAR3{1} CVAR4{1} RLBDIFF{1}
```

```
ASSOCIATE CEQ

# -0.125784814 -0.026344580 -0.197419823 $
  0.228154705  0.000955895

FRML(EQUATION=CEQ) CFM

* EQUATION FOR IMPORTS (NON-OIL)

EQUATION MEQ LOGM
# CONSTANT LOGM{1} CDIFFM MVAR3 LOGGDPE{2}

ASSOCIATE MEQ
# -2.33695833 0.80525152 0.79118404 20.13666059 0.36989006

FRML(EQUATION=MEQ) MFM

* IDENTITY FOR EXCHANGE RATE

FRML(IDENTITY) EERID EER = XEER*(1 + (RLB-RSW)/400)

* IDENTITY FOR COMPETITIVENESS

FRML(IDENTITY) COMPID COMP = WSI/(PROD*WWPI/EER)

* IDENTITY FOR CAPITAL ACCOUNT

FRML(IDENTITY) OJID OJ = OJ{1} + BAL - DRESV

* IDENTITY FOR EXPENDITURE MEASURE OF GDP

FRML(IDENTITY) GDPEID GDPE = C + IFP  + KII - KII{1} + G + $
  X - M + O - FACTOR

* GOVT. DEBT

FRML(IDENTITY) GOVTBID GOVTB = GOVTB{1} + DGG

* IDENTITY FOR GILTS AND OTHER GOVT. DEBT

FRML(IDENTITY) DGGID DGG = PSBR -(MONEY-MONEY{1}) + DRESV

* GROUP EQUATIONS AND IDENTITIES IN MODEL

GROUP GKSMALL PCRATIOFM WAGESFM LOGXFM LOGPXFM $
  KIFM KIIFM ETFM CFM MFM EERID>>EER COMPID OJID GDPEID>>GDPE $
  GOVTBID DGGID


*PROJECTIONS FOR RLB, PSBR AND BAL

DATA(UNIT=INPUT) 1993:1 1993:4 RLB
6.33 6.33 6.33 6.33

LINREG(NOPRINT, FRML=PSBREQ) PSBR
# PSBR{1 TO 3}


LINREG(NOPRINT, FRML=BALEQ) BAL
# BAL{1 TO 8}


* PROJECTIONS FOR EXOGENOUS VARIABLES
* G, O, YD, RSW, WWPI, XWM, POILWC, WGNP, MONEY, DRESV, TAX, TIME

* PROJECTIONS FOR G

LINREG(NOPRINT, FRML=GFM) G
# G{1 TO 3}

* PROJECTIONS FOR O

LINREG(NOPRINT, FRML=OFM) O
# O{1 TO 3}

* PROJECTIONS FOR YD

LINREG(NOPRINT, FRML=YDFM) YD
# YD{1 TO 3}
```

```
* PROJECTIONS FOR RSW

LINREG(NOPRINT, FRML=RSWFM) RSW
# RSW{1 TO 3}

* PROJECTIONS FOR WWPI

LINREG(NOPRINT, FRML=WWPIFM) WWPI
# WWPI{1 TO 3}

* PROJECTIONS FOR XWM

LINREG(NOPRINT, FRML=XWMFM) XWM
# XWM{1 TO 3}

* PROJECTIONS FOR POILWC

LINREG(NOPRINT, FRML=POILWCFM) POILWC
# POILWC{1 TO 3}

* PROJECTIONS FOR WGNP

LINREG(NOPRINT, FRML=WGNPFM) WGNP
# WGNP{1 TO 3}

* PROJECTIONS FOR MONEY

LINREG(NOPRINT, FRML=MONEYFM) MONEY
# MONEY{1 TO 8}

* PROJECTIONS FOR DRESV

LINREG(NOPRINT, FRML=DRESVFM) DRESV
# DRESV{1 TO 8}

* PROJECTIONS FOR TREF

LINREG(NOPRINT, FRML=TREFFM) TREF
# TREF{1 TO 3}

* PROJECTIONS FOR TAX

LINREG(NOPRINT, FRML=TAXFM) TAX
# TAX{1 TO 3}

* PROJECTIONS FOR TIME

LINREG(NOPRINT, FRML=TIMEFM) TIME
# CONSTANT TIME{1}

* INPUT VALUES FOR XEER

DATA(UNIT=INPUT) 1993:1 1993:4 XEER
79.8 79.8 79.8 79.8

* IDENTITIES FOR TRANSFORMATIONS

FRML(IDENTITY) PRODID PROD = 1.0057**(TIME - 32)

* PRICES

FRML(IDENTITY) LOGPCID LOGPC = LOG(PC)
FRML(IDENTITY) TREFID LOGTREF = LOG(TREF)
FRML(IDENTITY) PCVAR6ID PCVAR6 = LOG(PC{1}/(WWPI{1}/EER{1}))
FRML(IDENTITY) PCVAR7ID PCVAR7 = LOG(GDPE{1}/PROD)

FRML(IDENTITY) PCFITID PC = EXP(PCRATIO + LOGPC{1})

* INTEREST RATES

FRML(IDENTITY) MOPCVARID MOPCRAT = LOG(MONEY/PC{1})
FRML(IDENTITY) GDPEVARID GDPEVAR = LOG(GDPE - 0)
FRML(IDENTITY) TIMELAGID TIMELAG = TIME - 32

* WAGES

FRML(IDENTITY) LOGWSIID LOGWSI = LOG(WSI)
FRML(IDENTITY) WSIRATID WSIRATIO = LOG(WSI/WSI{1})
```

```
FRML(IDENTITY) WAGEV2ID WAGEVAR2 = LOG(WSI/(1.0057*PC)) - LOG(WSI{1}/PC{1})
FRML(IDENTITY) WAGEV3ID WAGEVAR3 = PCRATIO - PCRATIO{1}

FRML(IDENTITY) WSIFITID WSI = EXP( LOGWSI{1} - 0.009703990 $
  + 0.03276270*WAGEVAR{1} - 1.070075715*WAGEVAR2{1} - 0.561039629*WAGEVAR3{1})

* EQUATION FOR EXPORT DEMAND

FRML(IDENTITY) XWMID LOGXWM = LOG(XWM)
FRML(IDENTITY) LOGCOMPID LOGCOMP = LOG(COMP)

FRML(IDENTITY) XFITID X = EXP(LOGX)

* EQUATION FOR EXPORT PRICES

FRML(IDENTITY) WSIVARID WSIVAR = LOG(WSI{1}/PROD)
FRML(IDENTITY) WWPIVARID WWPIVAR = LOG(WWPI/EER)

FRML(IDENTITY) PXFITID PX = EXP(LOGPX)

* EQUATION FOR FIXED INVESTMENT

FRML(IDENTITY) KIVAR2ID KIVAR2 = LOG(KI/GDPE)
FRML(IDENTITY) KIVAR3ID KIVAR3 = LOG(GDPE/GDPE{1})
FRML(IDENTITY) KIVAR4ID KIVAR4 = LOG((WSI{1}*ET{1})/(PC{1}*(1.0057**TIME)))

FRML(IDENTITY) KIFITID KIFITTED = KIFITTED{1} + GDPE{1}*EXP(-0.616613709 $
  + 0.923349555*KIVAR1{1} + 0.035927732*KIVAR2{1} + 1.857239412*KIVAR3{1} $
  + 0.042422053*KIVAR4)

FRML(IDENTITY) KIID KI = KIFITTED


* FLOW OF GROSS PRIVATE INVESTMENT

FRML(IDENITITY) IFPID IFP = KIFITTED - KIFITTED{1} + 4610*1.0086**(TIME-100)

* EQUATION FOR INVENTORIES

FRML(IDENTITY) RLBV1ID RLBVAR1 = 400*(EER/(EER - 1))
FRML(IDENTITY) RLBV2ID RLBVAR2 = 400*((WWPI*EER{1})/(WWPI{1}*EER) - 1)
FRML(IDENTITY) RLBV3ID RLBVAR3 = 400*(WSI/(WSI{1} - 1))

FRML(IDENTITY) RREALID RREAL = 157.1804378 + 0.8709958*RLB{1} $
  - 0.3832469*RLBVAR1{1} + 0.0047546*RLBVAR2{1} - 0.0025148*RLBVAR3{1}

FRML(IDENTITY) LOGKIIID LOGKII = LOG(KII)
FRML(IDENTITY) KIIGDPEID KIIGDPE = LOG(KII/GDPE)

FRML(IDENTITY) KIIFITID KII = EXP(KIIRATIO + LOGKII{1})

* EQUATION FOR EMPLOYMENT(WHOLE ECONOMY)

FRML(IDENTITY) LOGETID LOGET = LOG(ET)
FRML(IDENTITY) ETVAR2ID ETVAR2 = LOG(ET{1}*PROD/GDPE{1})
FRML(IDENTITY) ETVAR3ID ETVAR3 = LOG(TREF/PC)
FRML(IDENTITY) ETVAR4ID ETVAR4 = LOG(WSI{1}/(PROD*PC{1}))

FRML(IDENTITY) ETFITID ET = EXP(ETDIFF + LOGET{1})

* EQUATION FOR CONSUMER SPENDING

FRML(IDENTITY) LOGCID LOGC = LOG(C)
FRML(IDENTITY) CVAR2ID CVAR2 = LOG(C/(YD/(PC/100)))
FRML(IDENTITY) CVAR3ID CVAR3 = LOG((PC/PC{1}) + 0.0057)
FRML(IDENTITY) CVAR4ID CVAR4 = LOG((YD/PC)/(YD{1}/PC{1}))
FRML(IDENTITY) RLBDIFFID RLBDIFF = RLB - RLB{1}

FRML(IDENTITY) CFITID C = EXP(LOGC{1} + 0.0057 - 0.125784814*CDIFF{1} $
  - 0.026344580*CVAR2{1} - 0.197419823*CVAR3{1} $
  + 0.228154705*CVAR4{1} + 0.000922160*RLBDIFF{1})


* EQUATION FOR IMPORTS (NON-OIL)

FRML(IDENTITY) CDIFFMID CDIFFM = LOG(C/C{1})
FRML(IDENTITY) MVAR3ID MVAR3 = LOG((KII/KII{1})/(KII{1}/KII{2}))
FRML(IDENTITY) LOGGDPID LOGGDPE = LOG(GDPE)
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FRML(IDENTITY) MFITID M = EXP(LOGM)

* IDENTITY FOR ADJUSTMENT TO FACTOR COST

FRML(IDENTITY) FACTORID FACTOR = 0.139228970*C + 0.053169319*IFP $
 -  0.005139745*X + 0.143336863*G

* GROUP INTO MODEL AND FORECAST

GROUP GKBIG PCRATIOFM WAGESFM LOGXFM LOGPXFM $
  KIFM KIIFM ETFM CFM MFM EERID>>EER COMPID OJID GDPEID>>GDPE $
  GOVTBID DGGID $
  PSBREQ>>PSBR BALEQ>>BAL $
  GFM>>G OFM>>O YDFM RSWFM>>RSW WWPIFM>>WWPI XWMFM>>XWM POILWCFM>>POILWC $
  WGNPFM>>WGNP MONEYFM>>MONEY DRESVFM TREFFM TAXFM TIMEFM $
  PRODID LOGPCID TREFID PCVAR6ID PCVAR7ID PCFITID>>PC $
  MOPCVARID GDPEVARID TIMELAGID $
  LOGWSIID WSIRATID WAGEV2ID WAGEV3ID WSIFITID>>WSI $
  XWMID LOGCOMPID XFITID>>X $
  WSIVARID WWPIVARID PXFITID>>PX $
  KIVAR2ID KIVAR3ID KIVAR4ID KIFITID KIID>>KI IFPID>>IFP $
  RLBV1ID RLBV2ID RLBV3ID RREALID LOGKIIID KIIGDPEID KIIFITID>>KII $
  LOGETID ETVAR2ID ETVAR3ID ETVAR4ID ETFITID>>ET $
  LOGCID CVAR2ID CVAR3ID CVAR4ID RLBDIFFID CFITID>>C $
  CDIFFMID MVAR3ID LOGGDPID MFITID>>M FACTORID>>FACTOR

SMPL 1993:1 1993:4
FORECAST(MODEL=GKBIG, PRINT)

* GRAPHS ETC. TO FOLLOW :-)

PRINT 1993:1 1993:4 GDPE BAL PC RLB PSBR
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